A JOURNAL OF NATURAL PHILOSOPHY, CHEMISTRY, AND THE ARTS.

VOL. VIII.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:

PRINTED BY W. STRATFORD, CROWN-COURT, TEMPLE-BAR, FOR

THE AUTHOR, No. 10, SOHO-SQUARE;

AND SOLD BY

ALL THE BOOKSELLERS IN THE UNITED KINGDOM.

1804.

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Of the Engravings the Subjects are; 1. Machine of considerable Power for clearing Roads of Mud, by Dr. Winterterbottom. 2. Mr. Robert Green's Hand Drill for Pins. 3. The Rev. E. Cartwright's Three-Furrow Plough. 4. Diagram for investigating the Figure of the Earth. 5. New Galvanic

vanio-Apparatus, convertible at Pleasure into one or more Plates. 6. Hydraulic Machine operating by the rotation of two Pinions, in a Water Veffel. 7. An improved Jib for a Crane, by Mr. Bramah. 8. New Filtering Apparatus, by Messrs Harman and Dearn. 9. Galvanic Apparatus. 10. An ancient Lock ufed in Egypt and Western Asia for above three thousand Years. 11. Developement of the Mechanifm of the Lock. 12. Sketch of the Orbits of the New Planets, by Jerome de Lalande. 13. Cryftals of Arseniated Copper, by Hauy. 14. Telegraph by the Human Figure. 15. Machine for levelling the Surface of Land, by Mr. David Charles. 16. Shaded Sections of a Clock, which strikes the Hours by simpler Mechanism, and with greater regularity than usual, by means of a Pendulum substituted in the Place of a Fly. 17. Specimen of a very curious antique Composition or Painting in coloured Glass, by an Art at present lost. 18. Improved Lamp for producing a strong Heat in Chemical Experiments, by Mr. Accum.

Soho Square, Auguft, 1804.

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MAY, 1804.

ARTICLE I.

Letter from C. Wilkinson, *Esq. on the Means of simplifying and improving the Galvanic Apparatus.*

To Mr. NICHOLSON.

SIR,

THE communication from your ingenious correspondent I. R. I. afforded me considerable pleasure. As you favoured me with his idea how the galvanic power may be increased to an immense degree, prior to its appearance in your valuable Journal, I have been induced to reflect upon various modes of galvanic arrangements. Introduction.

If the whole apparatus be made to consist only of a single plate reflected backwards and forwards, so as to expose an immense surface, no plate of copper would be required, only taking care, which in this instance would be easily effected, that one side of the plate only should be exposed to the action of the acid. For, from various experiments, I am persuaded that the other metal answers no other electrical purpose, than to guard and protect the zinc side, to which it is soldered from being acted upon by the acid. Even cement answers very well, if we only preserve through the cement a good conducting medium to the zinc. Thus with a zinc plate of eight inches diameter, if only a piece of copper the size of a half- penny

In apparatus of one single plate, the coating of copper is unnecessary.

Cement will do as well.

penny be foldered in its centre, and all the reft of that furface, which is ufually foldered to copper or filver, be well covered with cement, equal effects would be obtained.

Proof in the Couronne de Taffes;

In a Couronne de Taffes I find the fame effects are produced, whether the copper be of a correfponding fize to the zinc plates, or, whether they be merely fimple copper or filver wires.

and in a pile.

Upon this principle I have conftructed a pile, and find it produces the fame effects, as if the whole furface were covered with copper; I purpofe foon to arrange a trough upon the fame principles, and I am perfuaded, that the more tedious and expenfive part of galvanic apparatus may thus be prevented.

The corrofion in a trough is greateft near the air.

When a trough has been fometime employed, upon removing the plates, the lofs of metal, I always obferve, does not take place uniformly over the furface, but in the upper part, which in the galvanic action is the moft expofed to the atmofphere, is the moft acted on, and towards the bottom, the metal is very little altered. In order to preferve a more equal action, I am now preparing a battery formed of plates of ten inches by two and a half, the longeft fide placed horizontally, and I am perfuaded, that this fized plate will produce more active effects than a plate of five inches fquare.

Hence a *pile of difhes* would be preferable.

It is well known, that in every galvanic operation, oxidation is produced, and if oxigen can be procured from the furrounding air, the effect would be more eafy than producing it from the decompofition of water. I have no doubt, that if a feries of zinc plates formed like large pewter difhes, were to be arranged in a pile-like form, infulated from each other, and the galvanic mixture to be poured in the hollow part; the lower fide being covered with cement, excepting that from the centre a piece of copper fhould be foldered, and fo projecting as to be in contact with the fluid in the cavity of the plate below; that a very powerful apparatus would thus eafily be formed.

On the fubftitution of air inftead of wet mediums by Dyckhoff.

In the latter part of your Journal I perufed with a confiderable degree of furprife, fome obfervations of a Mr. Dyckhoff relative to the fubftitutions of thin ftrata of air, inftead of wet media. As the refults of his experiments appeared fo contrary in principle to any I had tried, I immediately repeated them in the manner he has defcribed, with interpofed

lentils of glafs, fo as to render the feparation very fmall. I then arranged feveral feries of plates of one inch, to plates of feven inches in diameter; becaufe he has not particularized the fize of the plates he employed. I arranged piles of thefe different fizes to the number of twenty pairs in each arrangement, and obferved not the flighteft galvanic effect, either by my tongue or by any other teft. I employed a very excellent condenfer; no influence at all was evinced; and, laftly, I fubjected it to the moft delicate electrofcope we have, viz. the mufcular fibres of a frog. Not the flighteft difturbance took place, although I fancy I have demonftrated, in the elements of galvanifm I have publifhed, that the fenfibility of this animal electrofcope is fifty thoufand times greater than that of the condenfer.

The experiment did not fucceed.

As to the charging of a Leyden phial, I am convinced it is perfectly erroneous to fuppofe it has ever been done. I have employed from fifty pair of plates to fifteen hundred, and never yet have produced any charge. Nor indeed could fuch be expected from the weak intenfity of the ftate of electricity in galvanic operations; for a jar cannot be charged until a fufficient quantum is accumulated to overcome the refiftance of the furrounding air, fo abfolutely requifite to the charging of a Leyden phial.

Conviction of the author that galvanifm cannot charge a Leyden jar.

I am, Sir,

Your's, &c.

C. WILKINSON.

ANNOTATION. W. N.

THE valuable obfervations in the preceding letter, will naturally fuggeft improvements to thofe who are employed in the conftruction of galvanic apparatus. It muft be a great advantage, that the expence of copper and the work of foldering, or placing it, will be almoft entirely faved. *Fig.* 1. *Plate* III. fhews a fimple method of difpofing a fingle plate of zinc of large furface in a trough. It is fuppofed to have been made fufficiently hot to bed itfelf in cement at the bottom of the box in which it is placed, and its two ends A and B are fecured in the fame way. The fhaded fpaces reprefent the cavity occupied by acid, and the dotted fpace is left empty.

Form of galvanic apparatus in a fingle piece.

 A com-

A communication may be made by a copper wire from the wet or corroded furface to the oppofite or dry furface, as fhewn at A and C.

Another of many pieces acting as one.

As it may be difficult to bend a very long piece of laminated zinc, and to keep it flat, fo as to give it a fecure and clofe fixture in the cement, I fhould prefer a number of ftrait pieces fcrewed together with varnifhed pieces of wood between their extremities: every fecond piece being perforated on the acid fide, to permit a free communication.

More particular account of the pile of difhes.

Mr. Wilkinfon's pile of difhes promifes to be very effectual, and of eafy conftruction. I fuppofe the difhes to be of zinc, either caft in a metallic mould or ftamped, and that each fhould be provided with three fhort copper legs, foft-foldered on; after which, the lower face of the difh fhould be well defended by varnifh or cement, and alfo the lower ends, but not the fides of the copper legs. Thus prepared, they might be eafily builded up, and charged by a proper funnel, and an appropriate meafure for dealing out the acid.

Galvanic trough of one metal convertible into a fingle plate, by varying the commu

In *Fig.* 2. I have ventured to offer the fketch of a trough, confifting of zinc plates only, principally becaufe it appears capable of being as fpeedily charged with acid as another trough, and may with facility and at pleafure be ufed either as one fingle plate, or as the ufual feries of plates, acting in fucceffion upon each other. Let A B reprefent a trough conftructed after the manner of Cruickfhank; excepting that the plates are of zinc only: and let the alternate fhaded cells be charged with diluted acid, while the dotted cells continue empty. This may, without difficulty, be done at one pouring, by means of a channel in the wooden fide of the trough.

Arrangement to produce the ufual effect of a large plate.

Whether this trough, fo charged, fhall act as one extended galvanic furface, or as a continued feries, will be governed by the copper wires of communication. If the former, then a long copper wire having branches defcending from it into every one of the acid cells muft be duly placed; and another wire parallel to, but not in contact with, the former, and having double branches defcending into all the empty cells, fo as to touch both the dry metallic furfaces, muft alfo be placed. Whenever a communication is made between thefe two principal wires or conductors, the galvanic energy will pafs through the medium of communication. I have not given a diagram, becaufe this difpofition is very obvious.

If

If the latter effect be required, namely, that the trough should act like a continued series, the connection of the troughs must be differently made. A number of clipping pieces or spring forks must be provided of copper wire, as represented by the curved lines *a*. *b*, *c*, *d*, &c. from each of which proceeds a third leg or branch, as seen in the figure. These are applied (most conveniently above, but) not so as to touch each other; the clipping part of each embracing the dry surfaces of a pair of the zinc pieces, which are acted upon by the same mass of acid, while the third leg is immersed in the acid of the next adjacent cell. Every one of their legs or branches is disposed towards the same region or part of space; by which means the acid of each compartment acting upon a pair of zinc plates, on one surface only, is made to communicate, by the interposition of copper, with the uncorroded side of the next pair in succession; and so on, exactly as in the common trough or pile. Arrangement for the effect of a succession of plates.

II.

Observations and Communications on the Dry Rot in Timber, made to the Society for the Encouragement of Arts.

(Concluded from p. 318 of Vol. VII.)

Second Letter from BENJAMIN JOHNSON, *Esq.*

SIR,

THE observations I sent yesterday were taken from different parts of my note-book, in haste, because the second Tuesday in December was past; for it was by accident I saw the advertisement on Saturday; but wishing not to be deficient in information, I trouble you again.

The leaves of the plant appearing exhausted and dead, is owing to their having imparted all their juices to the wood, which changed it to a fungus, and not to a powder, like rottenness from length of time. A more full account of the plant that occasions the dry rot;

The *Boletus Lachrymans* is of the fungus tribe, and is one of the few that have leaves, as the miseltoe, &c.

Nothing is more easy than to prevent the damage from the plant. Besides what I said yesterday, I am positive that a tile laid

laid close along the walls round the room, would prevent the growth of the plant, even without mortar; and perhaps it is only necessary where the walls are next to the air.

and the method of cure.

Charring the ends of the joists for a few inches, and charring the side of the wainscot at bottom, next to the walls would be sufficient; for the plant cannot adhere to any thing but wood, and that possessed of its natural juices, to a certain degree; so that I question if old dry oak would receive it.

All the white soft woods, as beech, poplars and deals, are for a long time ready to receive it. Repairing the damage with fresh wood, without removing the earth and plant, is only feeding the evil.

The plant is of the creeping kind, and cannot rise two inches; so that wood in all cases, must be in contact with the earth to support it.

A fungus broader than the palm of one's hand, and an inch or more in thickness, is commonly seen at the bottom of an old post, on the surface of the earth; but it is not easy to discern whether the wood or the earth furnishes the matter; so true is the observation of Muller:—"*Dans l'étude de la nature, on peut nous comparer à de petits enfans qui commencent à ouvrir les yeux; nous voulons parler beaucoup, et nous ne faisons que bégayer.*"

I am, Sir,

Your most obedient Servant,

BENJAMIN JOHNSON.

Ipswich, Dec. 21, 1799.

To the Secretary.

N. B. The qualities of this plant are unknown to most English botanists, as appears from their publications; but they are known to the Germans, who have habitually used more wood in their buildings than we have.

Third Letter from the Same.

SIR,

Assured that the pursuits of the Society for the Encouragement of Arts, &c. aim at the full investigation of whatever they propose for the public benefit, I cannot persuade myself that I am troublesome in going a little further into this subject.

I had

I had lately a converfation with an old friend, who fhowed me two parcels of rotten wood, from an oak barn floor, laid about fixteen years ago. After lying twelve years it fhook upon the joifts. On examination, it was found to be rotted in various parts, and the planks, two inches and a half in thicknefs, were nearly eaten through, though the outfide was gloffy, and without blemifh. The joifts, and a large middle beam were laid at the ends, in brick and mortar, to create a firm level. No earth was near the wood; and he thinks that no air could find a paffage. The rottennefs was partly an impalpable powder, of the colour of Spanifh fnuff, and other parts were black, as if burnt; the reft was clearly a fungus.

Some account of a barn floor decayed.

This gentleman is a perfon of undoubted veracity; but a nice and exact obfervation is neceffary in fuch examinations. He thought nothing of any plant, and it is likely there was none of the Boletus; fo that my affertion that it was always to be found, was rather too fyftematic.

It does not appear to have been caufed by any plant.

I afked him if the timber was dry when laid down. He could not however fay that had been particularly adverted to. It had been fawed from a large oak, and was, as he thought, in all refpects proper for a barn floor. As this feems not the operation of the Boletus, how did it happen?

We know that the oak, when in vegetation, is fubject to what I fhall call an exudation of juices, which produces the fungus, named the Agaric of the oak, with which the Druids of old played many tricks. The oak, then, if fawed into thick quantities, may emit thefe fame juices, as the progreffive courfe of nature to its entire decay.

On the decay of oak timber.

We have all feen oaks of vaft fize and ancient record, with a great part of the outfide whole, and all the infide gone; perhaps the work of a century. In all hollow trees fungus is difcoverable. To ufe a law term, it is a *mifnomer* to call it dry-rot; for the rotting principle is in moifture.

I had never feen the rot upon fo large a fcale as in timber, till lately. The prevention, then, of beams, rafters, large joifts, and pofts, put into the earth, from decay by the rot, is in charring only, which will dry up all the fungus juices of wood in large fubftance. Paint, or a bituminous preparation, may probably ftop up the pores, and prevent the rot in flight work, where the treatment I before obferved, with fire, might be incommodious, as in half-inch wainfcot, &c.*

The preparation of wood for preferving it, fhould be either charring, or bituminous paint.

The

Durability of charcoal.

The incorruptibility of charcoal is attested by undoubted historical facts, at the destruction of the famous temple at Ephesus. It was found to have been erected on piles that had been charred; and the charcoal in Herculaneum, after almost 2000 years, was entire and undiminished.

I am, Sir,

Your most obedient Servant,

BENJAMIN JOHNSON.

Ipswich, December 26.

Letter from RICHARD RAMSDEN BRAMLEY, *Esq. of Leeds, relative to the Dry Rot in Timber.*

To CHARLES TAYLOR, Esq.

SIR,

Introduction.

I take the liberty of inclosing to your care an Essay on the Dry Rot in Timber, which you will be so obliging as to lay before the Society for the Encouragement of Arts, &c. Should this Essay be deemed worthy of attention, or should any farther notice be necessary respecting it, every information that may promote the views of your respectable Society will be given with pleasure by,

Sir,

Your most obedient Servant,

R. RAMSDEN BRAMLEY.

Leeds, Aug. 26, 1799.

As the Society for the Encouragement of Arts, &c. have for some years offered a premium for the discovery of the cause occasioning the dry rot in timber, of which, it seems, no satisfactory account has yet been received; should the following prove so, it will give the author much pleasure. To bring the matter to the test by experiments, would require the observation of a long period, and in selected situations.

Wood, used for the general purposes of man, is cut down at different periods; and although it may be felled at the proper season, or when most free from sap or moisture, it is not always to be effected.

Even admitting it to have been cut down in the most favourable situation, it still abounds with such an extra proportion of

moiſture, as to require a regular expoſure to the air, prior to its being applied to uſe, if we wiſh to guard againſt that ſhrinking which always takes place, where this precaution has not been taken.

Facts and obſervations concerning the dry rot in timber, and its cure.

Although the fir kind contains leſs of this watery portion, yet it aſſuredly poſſeſſes a conſiderable ſhare; and it is in this ſpecies, I apprehend, that the evil called the dry rot moſt generally occurs, as from the facility of working the ſame, it is moſt generally applied in buildings.

But ſuppoſing it to be fir, or any other ſpecies; wood felled when abounding with any extra proportion of ſap, and applied to uſe without the proper ſeaſoning or expoſure to a free current of air, until ſuch extra moiſture as has had time to exhale, is moſt liable to the diſeaſe in queſtion; and the cure, or principal prevention againſt it, would be the precaution of felling all wood *only* at the proper ſeaſon, or when the ſap is not in circulation. The next mode of prevention would be to uſe ſuch wood only as has been for a conſiderable period expoſed to the influence of a free current of air, or where convenience will admit, to that of air heated to a moderate degree; ſuch air extracting with greater facility the incloſed moiſture, and in a more certain ratio than the irregularity of our atmoſphere will allow.

In all rapidly-improving countries, this evil is likely to be an increaſing one, as the current demand for wood generally exceeds the ſupplies laid by in ſtore, ſo as to be applied to uſe in regular ſucceſſion, after being properly ſeaſoned.

Another cauſe that affects all wood moſt materially, when not fully dried, is the application of paint, the nature of which prevents all exhalation, and confines the incloſed moiſture, till it occaſions a fermentation through the whole fibrous ſyſtem of the wood, and brings on a premature ſtate of decompoſition, or the dry rot.

A ſimilar evil may be induced, in conſequence of any newly-finiſhed building having all the doors and windows ſhut up, and that for ſome length of time, particularly in moiſt weather. The wood, even though unpainted, is thus frequently placed in an atmoſphere more charged with vapour than its own internal contents, and is conſequently in an imbibing inſtead of an exhaling ſtate, and tending to decay. Wood placed in dampiſh ſituations, and the ends of timbers near to moiſt walls, ſuffer from ſimilar cauſes.

What

Facts and observations concerning the dry rot in timber, and its cure.

What particularly attracted my observation to the circumstances was this, that both oak and fir posts were brought into this premature state of decay, from their having been painted prior to the due evaporation of their moisture; and then extending the observation, and tracing the history of other wood affected in a similar manner, I am convinced that the evil frequently thus originates, and its prevention would be in using timber, previously well dried and seasoned.

RICHARD RAMSDEN BRAMLEY.

SIR,

A considerable time has elapsed since I furnished you with some observations relative to the dry rot in timber, and having been since engaged busily in draining from 4 to 5000 acres of ground, further ideas on the subject of the dry rot have in the interim recurred to me from the work I have been engaged in, which, if the respectable Society to which you are Secretary think worthy attention, they may add to, or connect with my former ideas, as may be deemed most useful. Where houses are troubled with damp walls, near to the earth's surface, it is generally, if not universally, occasioned by the percolation of water from the higher adjoining ground, which, thus intercepted in its current, attempts to follow the general hydrostatic law, of elevating itself, by the syphon line, to a height equal to that from whence it has its origin. Thus, in houses differently situated, we see the damp arising, to varying degrees of height, on the walls; and those are probably all corresponding to the height at which the moisture circulates in the adjoining ground. At its first entrance to the building, and whilst the moisture is in small quantity, the excavated part of the foundation wall may absorb, and gradually quit such proportion; but the excess, as is generally the case in moist weather, exceeding that power, the foundation stones are then saturated in a more rapid proportion than the adjoining rarified internal atmosphere can evaporate: the watery particles then creep up, in degrees proportionate to the ascent from which they originally descended, excepting when prevented, or driven off by the superior heat of the adjoining rooms, when, in addition to the disagreeable damp they cause, they frequently occasion considerable damage to pictures, furniture, &c. Drains laid out athwart the ascending ground, with a very

very flight defcent or fall, and made of the depth of one yard for each yard of afcent, and from the foundation until equal to the height that fuch damp ever rifes, would, there is little doubt, completely fecure the houfe and furniture from the inconveniences hitherto fuftained, and would generally prove an effectual prevention to moft cafes of the dry rot, where it originates in extreme moifture. I am of opinion that the fungus which pervades decaying wood is not the firft caufe, but an attendant on the peculiar ftate to which fuch wood has been reduced by prior caufes. The diffeminated feeds finding a proper bed, or *nidus*, like the mufhroom, toad-ftool, &c. fix there their abode, and pervade the whole fubftance, thus accelerating the general law of Providence, which tends to make all matter re-productive. Facts and obfervations concerning the dry rot in timber, and its cure.

Cellars, or fuch other places, fhould be drained in the manner I have above mentioned, by taking off the percolating water, prior to its gaining admiffion to or contact with the walls; and it is probable that, in moft cafes, a fingle drain will have complete effect; it would affuredly do fo, if it was not for the variation of the earth's internal ftrata, which are not eafily difcernible. If attention to this rule was paid prior to the building any new ftreets in towns, it would prove effentially ufeful.

I am, with efteem,

Dear Sir,

Your's truly,

Leeds, June, 1803. R. RAMSDEN BRAMLEY.

To Mr. Charles Taylor.

The Society have been informed, that mortar made of lime from burnt chalk is much more deftructive to timber than ftone lime, or that burnt from lime-ftone. Chalk lime attracts moifture; and communicating it to any timber which it touches, occafions its decay.

Sea fand is alfo prejudicial, if made into mortar, from a fimilar quality of attracting moifture from the atmofphere: this may in fome degree be corrected by wafhing the fand well in frefh water, where good fand cannot be procured.

Good mortar, where any is required to be in contact with timber, may be made from a mixture of ftone lime frefh burnt, and

and river ſand, to which a very ſmall quantity of common brown, or yellow iron ochre, ſhould be added, and well incorporated therewith.

III.

On the Figure of the Earth. By PEREGRINUS PROTEUS.

To Mr. NICHOLSON.

SIR,

IN ſome of your late Journals I obſerve a paper on the figure of the earth, by Mr. John Playfair, profeſſor of mathematics in the Univerſity of Edinburgh, containing ſeveral new theorems, and ingenious remarks, on a ſubject which has engaged the attention of the firſt mathematicians of Europe ſince the days of Newton. On reading it, I was led to examine the properties of ſpheroidal triangles, and to inveſtigate the problem, propoſed by the author, for determining the dimenſions of the earth from the length of the ſtraight line or chord joining two places whoſe geographical ſituations are given. Theſe are intended to form the principal ſubject of this letter; but, before I proceed to them, I beg leave to make a few obſervations on that paper, without any view to cavil, or detract from its real merits.

Obſervations upon Profeſſor Playfair's memoir on the figure of the earth.

After taking notice of the diſagreement in the compreſſions of the terreſtrial ſpheroid, which reſult from the compariſon of different meaſurements, he aſſigns, as the principal reaſon for this inconſiſtency, the local irregularities in the direction of gravity, ariſing in ſome ſituations from the attraction of mountains, and in others from the unequal denſity of the materials under, and not far from, the ſurface of the earth. That the firſt has a ſenſible effect on the plumb-line has been proved by accurate and undeniable experiments; the ſecond is an ingenious and probable conjecture, which the ſurveys carrying on in Great Britain and France will afford data to refute or confirm. But though the former may operate in the general ſurvey of a country, where the obſerver has not his choice of ground, it has always been avoided as much as poſſible in meaſurements made for the expreſs purpoſe of determining the figure of the earth; and though the latter may produce ſome perceptible difference

difference in obfervations made in nearly the fame latitudes, does the author think it fufficient to account for the great difagreement in the refults from the comparifon of diftant obfervations? Is it not much more probable, without giving up the elliptic figure, that fome of the obfervers may have ufed different ftandard meafures from the reft, or not made proper allowances for the alteration of their lengths in different temperatures? In fhort, this circumftance appears to me fufficient to account for fome fmall local irregularities, but wholly inadequate to explain the great differences in the general refults.

Obfervations upon Profeffor Playfair's memoir on the figure of the earth.

The author then proceeds to point out feveral methods of calculating the dimenfions of the earth from terreftrial meafurements. The firft applies to the cafe where two arches of the meridian are given in different latitudes, which, under the moft favourable circumftances, is incomparably the moft accurate that can be employed. The rules he gives are certainly very fimple, and in fome refpects new; but he feems to be miftaken when he afferts, that the calculation muft be made by rules quite different from thofe that have been hitherto given. Euler's * is effentially the fame with his own; and Du Séjour, Legendre, Delambre, &c. have given many accurate theorems, which may be applied to this purpofe. The fecond method is, from comparing a degree of the meridian in any latitude with a degree of the curve perpendicular to the meridian in the fame latitude; and the third from the meafures of degrees of the curve perpendicular to the meridian in different latitudes. His theorems for both are very accurate and fimple. But the principal novelty of Mr. Playfair's paper is, the method he propofes of finding the figure of the earth from the length of a ftraight line or chord joining two places whofe geographical fituations are given. As he has left the folution of this problem to fome future occafion, the following perhaps may not be unacceptable:

Let PAO (*Plate* III. *Fig.* 1.) reprefent one quarter of the ellipfis, by the revolution of which round the femi-conjugate axis PC, half the terreftrial fpheroid is generated. Let C be the center of the earth, P the pole, CO the radius of the equator $=a$, CP half the polar axis $=b$, and $c=$ the compreffion at the poles, or the excefs of a above b. Let A in the

Solution of the problem for finding the figure of the earth, from the length of a chord joining two known places, &c.

* Memoires de L'Academie Royale des Sciences Belles Lettres a Berlin, 1753.

meridian

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

meridian PAO be one of the extremities of the measured chord; and B in the meridian P′B the other extremity; let AD, BF be drawn perpendicular to CP, BE perpendicular to the plane CPAO, and let AB, AE, FE be joined. Then will AE^2 be $=(CD-CF)^2+(AD-FE)^2=CD^2+AD^2\times CF^2+FE^2-2CD\times CF-2AD\times FE$, and $AB^2=AE^2+BE^2=CA^2+CB^2-2CD\times CF-2AD\times FE$.

Now let λ, ϕ be the latitudes of A and B expressed in decimals of the radius 1, ω the difference of longitude or the angle BFE, and D the length of the measured chord $=$ AB. Then from the properties of the ellipsis we have

$$CA^2=\frac{a^2 \text{ cos. } \lambda^2+b^2 \text{ sin. } \lambda^2}{a^2 \text{ cos. } \lambda^2+b^2 \text{ sin. } \lambda^2}=a^2-2ac \text{ Sin } \lambda^2$$

$$AD=\frac{a^2 \text{ cos. } \lambda}{\sqrt{(a^2 \text{ cos. } \lambda^2+b^2 \text{ sin. } \lambda^2)}}=a \text{ cos. } \lambda+c \text{ cos. } \lambda \text{ sin. } \lambda^2$$

and $CD=\dfrac{b^2 \text{ sin. } \lambda}{\sqrt{(a^2 \text{ cos. } \lambda^2+b^2 \text{ sin. } \lambda^2)}}=a \text{ sin. } \lambda-c \text{ sin. } \lambda$ $(2-\text{sin. } \lambda^2)$, neglecting the powers of c higher than the first, because c is very small in comparison of a. Whence by substitution, and putting $\delta^2=2a^2\ (1-\text{sin. } \phi \text{ sin. } \lambda-\text{cos. } \phi \text{ cos. } \lambda \text{ cos. } \omega)$, we obtain the following equation;

$$\delta^2-c\left(4a(\text{sin. } \lambda-\text{sin. } \phi)^2-\frac{\delta^2}{a}(\text{sin. } \lambda^2+\text{sin. } \phi^2)\right)=D^2,$$

and by extracting the square root of each side, and rejecting the square, cube, &c. of c, there results,

$$\delta-c\left\{\frac{2a}{\delta}(\text{sin. } \lambda-\text{sin. } \phi)^2-\frac{\delta}{2a}(\text{sin. } \lambda^2+\text{sin. } \phi^2)\right\}=D,$$

$$\text{or } a+c\left\{\tfrac{1}{2}(\text{sin. } \lambda^2+\text{sin. } \phi^2)-\frac{2a^2}{\delta^2}(\text{sin. } \lambda-\text{sin. } \phi)^2\right\}=\frac{Da}{\delta}$$

This equation may be otherwise expressed thus; let a spherical triangle be constructed, having two sides equal to the polar distances of A, B, and contained angle $=$ their difference of longitude; whence find the third side, which put $=\vartheta$. Then will sin. ϕ sin. λ + cos. ϕ cos. $\lambda\times$ cos. $\omega=$ cos. ϑ, and $1-\text{sin. } \phi \text{ sin. } \lambda-\text{cos. } \phi \text{ cos. } \lambda \text{ cos. } \omega=1-\text{cos. } \vartheta=2 \text{ sin. } \tfrac{1}{2}\vartheta^2$; therefore $\delta=2a \text{ sin. } \tfrac{1}{2}\vartheta$, and $D=2a \text{ sin. } \tfrac{1}{2}\vartheta+c\left\{\text{sin. } \tfrac{1}{2}\vartheta\ (\text{sin. } \lambda^2\times\text{sin. } \phi^2)-\dfrac{(\text{sin. } \lambda-\text{sin. } \phi)^2}{\text{sin. } \tfrac{1}{2}\vartheta}\right\}$. — The value of δ is manifestly equal to the length of a straight line joining two places, whose latitudes are λ, ϕ, and difference of longitude ω, on a sphere, whose radius is a.

From

From this equation the following method of determining the solution of the figure of the earth is deduced. Let l be the length of a measured chord, and λ, ϕ, ω the latitudes and difference of longitude of its extremities; find δ as above, and let $m = 2$ fin. $\frac{1}{2}\delta$, and $n =$ fin. $\frac{1}{2}\delta$ (fin. $\lambda^2 +$ fin. $\phi^2) - \frac{(\text{fin. } \lambda - \text{fin. } \phi)^2}{\text{fin. } \frac{1}{2}\delta}$.

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

Then if we reject all the powers of c higher than the first, we shall have the simple equation $ma + nc = l$. In like manner find a similar equation $m'a + n'c = l'$, corresponding to any other chord whose length is l', and there will result $a = \frac{n'l - nl'}{mn' - m'n}$, and $c = \frac{m'l - ml'}{mn' - m'n}$. The approximation may be easily carried further by including the second power of c, and thus finding an equation of the form $ma + nc + pc^2 = l$; but this labour would be useless, as the method itself does not admit of greater accuracy. If $\phi = \lambda$ the equation becomes $\frac{D}{2 \text{ fin. } \frac{1}{2}\delta} = a + c$ fin. ϕ^2, as is found by Mr. Playfair in § 31.

From the first equation a rule may be easily derived for calculating the difference of longitude of two places, when their latitudes and distance are given. For by transposition we have

$$\delta^2 \left(1 + \frac{c}{a}(\text{fin. } \lambda^2 + \text{fin. } \phi^2\right) = D^2 + 4ac\,(\text{fin } \lambda - \text{fin } \phi)^2,$$

and by division, and rejecting the powers of c higher than the first $\delta^2 = D^2 + \frac{c}{a}\Big(4a^2\,(\text{fin } \lambda - \text{fin.} \phi)^2 - D^2\,(\text{fin.} \lambda^2 + \text{fin.} \phi^2)\Big)$: but δ^2 is $= 2a^2\,(1 - \text{fin. } \lambda \text{ fin. } \phi - \text{cof. } \lambda \text{ cof. } \phi \text{ cof. } \omega)$, therefore cof. $\omega = \frac{1 - \frac{D^2}{2a^2} - \text{fin. } \lambda \text{ fin. } \phi}{\text{cof. } \lambda \text{ cof.} \phi} - \frac{c}{a}\Big(2\,(\text{fin. } \lambda - \text{fin. } \phi)^2 - \frac{D^2}{2a^2}\,(\text{fin. } \lambda^2 + \text{fin. } \phi^2)\Big)$ and putting $\frac{1 - \frac{D^2}{2a^2} - \text{fin. } \lambda \text{ fin.} \phi}{\text{cof. } \lambda \text{ cof. } \phi}$ $=$ cof. ω' we have $= \omega' + \frac{c}{a} \times \frac{2\,(\text{fin. } \lambda - \text{fin. } \phi)^2 - \frac{D^2}{a^2}}{\text{fin. } \omega'}$ $\frac{(\text{fin } \lambda^2 + \text{fin. } \phi^2)}{}$, which rule may be thus expressed. Let there be a spherical triangle, having two of its sides equal to the polar distances of the places, and the third side d such that

fin.

Solution of the problem for finding the figure of the earth from the length of a chord joining two known places, &c.

$\text{fin.}\ \frac{1}{2} d = \frac{D}{2a}$: find the angle ω' contained between the polar diftances, and the difference of longitude ω will be $= \omega' + \frac{2c}{a} \times \frac{\text{cof.}\ \frac{1}{2} d^2\ (\text{fin.}\ \lambda^2 + \text{fin.}\ \phi^2) - 2\ \text{fin.}\ \lambda\ \text{fin}\ \phi}{\text{fin.}\ \omega'}$.

The latitude ϕ may alfo be found from the fame equation, when λ, ω and D are given. For if the bafe of a fpherical triangle be $= d$, the two other fides $= 90° - \lambda$, $90° - \phi'$, and the contained angle $= \omega$, the cof. ω will be $=$

$\frac{1 - \frac{D^2}{2a^2} - \text{fin.}\ \lambda\ \text{fin.}\ \phi'}{\text{cof.}\ \lambda\ \text{cof.}\ \phi'}$. Now let $\phi = \phi' + x$, where x muft be very fmall, and cof. λ cof. ϕ' there refults cof. $\omega' =$ cof. ω $+ \frac{(\text{cof.}\ \lambda\ \text{fin.}\ \phi'\ \text{cof.}\ \omega - \text{fin.}\ \lambda\ \text{cof.}\ \phi')}{\text{cof.}\ \lambda\ \text{cof.}\ \phi'} \times \text{fin.}\ x$: confequently

$$\frac{(\text{cof.}\ \lambda\ \text{fin.}\ \phi'\ \text{cof.}\ \omega - \text{fin.}\ \lambda\ \text{cof.}\ \phi')}{\text{cof.}\ \lambda\ \text{cof.}\ \phi'} \times \text{fin.}\ x = \frac{c}{a}$$

$$\left(2\,(\text{fin.}\ \lambda - \text{fin.}\ \phi')^2 - \frac{D^2}{2a^2}\ (\text{fin.}\ \lambda^2 + \text{fin.}\ \phi'^2) \right) \text{ nearly,}$$

and $x = \frac{c}{a} \times$

$$\frac{\text{cof.}\ \lambda\ \text{cof.}\ \phi' \left(2\,(\text{fin.}\ \lambda - \text{fin.}\ \phi')^2 - \frac{D^2}{2a}\,(\text{fin.}\ \lambda^2 + \text{fin.}\ \phi'^2)\right)}{\text{cof.}\ \lambda\ \text{fin.}\ \phi'\ \text{cof.}\ \alpha - \text{fin.}\ \lambda\ \text{cof.}\ \phi'}.$$

From the inveftigation of Mr. Playfair's problem, therefore, we have obtained very accurate rules for finding ω from λ, ϕ and D, and ϕ from λ, ω, D.

Now in order to find an equation expreffing the relation between the latitudes, difference of longitude, and one of the azimuths, let AL be perpendicular to the meridian PAO in A meeting FE in L, and BK perpendicular to AL. Join KE, and the angle BKE will be equal to the fpheroidal angle OAB, and BFE equal to the angle APB or difference of longitude. Let OAB $=$ BKE $=$ A, BFE $= \omega$, and λ, ϕ as before, then will LE be $=$ (CD $-$ CF) cotang. λ + FE $-$ AD, KE $=$ (CD $-$ CF) cof. λ + (FE $-$ AD) fin. λ, and cotang. A $= \frac{KE}{BE}$.

Whence by fubftituting the valves of CD, CF, AD, BF given above, and rejecting the powers of c higher than the firft, there refults

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results cot. $A = \frac{\text{cos. } \phi \text{ sin. } \lambda \text{ cos. } \omega - \text{sin. } \phi \text{ cos. } \lambda}{\text{cos. } \phi \text{ sin. } \omega} - \frac{2c}{a} + \frac{(\text{sin. } \lambda - \text{sin. } \phi) \text{ cos. } \lambda}{\text{cos. } \phi \text{ sin. } \omega}$. But if ω be the vertical angle of a spherical triangle, and $90^\circ - \phi$, $90^\circ - \lambda$ the sides; also A' the supplement of the angle opposite the side $90^\circ - \phi$; then will cot. $A' = \frac{\text{cos. } \phi \text{ sin. } \lambda \text{ cos. } \omega - \text{sin. } \phi \text{ cos. } \lambda}{\text{cos. } \phi \text{ sin. } \omega}$, and consequently cot. $A =$ cot. $A' - \frac{2c}{a} + \frac{\text{cos. } \lambda (\text{sin. } \lambda - \text{sin. } \phi)}{\text{cos. } \phi \text{ cos. } \omega}$,

Whence as A, A' are nearly equal, we obtain $A = A' + \frac{c}{a}$. $2 \text{ sin. } A'^2 \times \frac{\text{cos. } \lambda (\text{sin. } \lambda - \text{sin. } \phi)}{\text{cos. } \phi \text{ cos. } \omega}$, which will be found abundantly accurate in practice, but if the square of c be retained, and $\frac{2 \text{ sin. } A'^2 \text{ cos. } \lambda (\text{sin. } \lambda - \text{sin. } \phi)}{\text{cos. } \phi \text{ sin. } \omega}$ be put $= M$, and sin. A'^2 cos. $\lambda \times \frac{\text{sin. } \lambda \text{ cos. } 2\lambda + 2 \text{ sin. } \lambda \text{ sin. } \phi^2 - \text{sin. } \phi}{\text{cos. } \phi \text{ sin. } \omega}$

$-$ cot. $A \times M^2 = N$, A will be $= A' + M \times \frac{c}{a} - N \times \frac{c^2}{a^2}$ more accurately. The rule may be thus expressed; let the colatitudes of the two places, and their difference of longitude form the sides, and contained angle of a spherical triangle, of which find the base angle at the place whose latitude is λ, and let it be $= \alpha'$, and the corresponding angle α of the spheroidal triangle will be $= \alpha' - \frac{c}{a} \times 2 \text{ sin. } \alpha'^2 \times \frac{\text{cos. } \lambda (\text{sin. } \lambda - \text{sin. } \phi)}{\text{cos. } \phi \text{ cos. } \omega}$. In like manner if β' be the angle of the spherical triangle at the place whose latitude is ϕ, the corresponding angle β of the spheroidal triangle will be found to be $= \beta' - \frac{c}{a} \times 2 \text{ sin. } \beta'^2 \times \frac{\text{cos. } \phi (\text{sin. } \phi - \text{sin. } \lambda)}{\text{cos. } \lambda \text{ cos. } \omega}$. Consequently $\alpha + \beta$ is $= \alpha' + \beta' - \frac{c}{a} \times \frac{\text{sin. } \lambda - \text{sin. } \phi}{\text{cos. } \phi \text{ cos. } \omega} \times 2 \text{ sin. } \alpha'^2 \text{ cos. } \lambda - \frac{c}{a} \times 2 \text{ sin. } \beta'^2 \times \frac{\text{cos. } \phi (\text{sin. } \phi - \text{sin. } \lambda)}{\text{cos. } \lambda \text{ cos. } \omega} =$ $\alpha' + \beta' - \frac{c}{a} \times \frac{2 (\text{sin. } \alpha'^2 \text{ cos. } \lambda^2 - \text{sin. } \beta'^2 \text{ cos. } \phi^2) (\text{sin. } \lambda - \text{sin. } \phi)}{\text{cos. } \lambda \text{ cos. } \phi \text{ cos. } \omega}$; but by spherics

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spherics fin. ω' cof. $\lambda =$ fin. β' cof. ϕ, therefore $\alpha + \beta = \alpha' + \beta'$ if we reject the powers of c higher than the first, which are insensible. Hence the principle laid down by Mr. Dalby, viz. that in a spheroidal triangle, of which the angle at the pole and the two sides are given, the sum of the angles at the base is the same as in a spherical triangle, having the same sides, and the same vertical angle, is verified, and therefore the concluding remark of Mr. Playfair is hasty and ungrounded. But perhaps Mr. P. in his solution retains the second power of c, and objects to Mr. Dalby's principle because its coefficient does not vanish except in particular cases. If so, the objection is frivolous, as the difference is so small as scarcely to be computed in the cases that occur in practice, and too small in any case to lead into error or deserve attention.

The preceding theorems for the solution of spheroidal triangles will be found extremely accurate, when applied to such as are described on the surface of the earth, on account of the smallness of c in comparison of a; and in like manner others may be deduced, when different parts of the triangle are supposed given. Thus if λ, α and D be given; let a spherical triangle be constructed with one side $= 90^\circ - \lambda$, another $= d$, such that fin. $\frac{1}{2} d = \frac{D}{2a}$, and the contained angle $= \alpha$; find the other side $90 - \phi'$, the angle at the pole ω', the other azimuth β' and we shall have equations of this form $\phi = \phi' + Pc$, $\omega = \omega' + Qc$, and $\beta = \beta' + Rc$, where P, Q, R are functions of λ, α, D, which may be derived from the foregoing equations by proper artifices. But the formulæ, except in particular cases, will not be found so simple as the former. These, however, and some new theorems applicable to trigonometrical surveys, I shall delay to some future communication. In the mean time, it may not be foreign to the subject to remark, that the arch of the meridian, said to have been lately measured in the Mysore country in the East Indies, by Brigadier Major Lambton, gives the degree, in latitude 12°.32′N. equal to 60494 fathoms; which compared with that of 60795 in latitude 47°.24′ N. gives $\frac{1}{310}$ for the compression at the poles, a quantity differing very little from the mean deduced from all the measures of degrees. But it must be confessed that there appear at present to be two very important objections against the accuracy of Major Lambton's measure. The Mysore, on account

Arch of the meridian measured in the Mysore country.

account of the irregularity of its furface and its uncertain elevation above the level of the fea, is an unfit country for afcertaining a nice point of this kind, however well fituated for connecting the eaftern and weftern fides of the peninfula by a geographical furvey; and the Major, from his account in the 7th volume of the Afiatic Refearches, feems to be fomewhat doubtful of the exact length of his chain. Neverthelefs it is probably to India that we muft look for the means of finally deciding this long contefted queftion. There, and there only, we find many tracts of country highly favourable to this purpofe; and it is to be hoped that the Eaft India Company, while anxious to afcertain the extent of its poffeffions, will not entirely neglect the interefts of fcience. —perhaps in fome degree uncertain from caufes ftated.

I am, Sir, &c.

PERIGRINUS PROTEUS.

Portfmouth, April 7, 1804.

IV.

Defcription and Drawing of a Hand Drill for fowing Peas, Beans, &c. Communicated to the Society of Arts, by the Inventor, Mr. ROBERT GREEN, *of Weftwratting, Cambridgefhire*.*

To CHARLES TAYLOR, Efq.

SIR,

I HAVE invented an engine to fow peas; with which I have fown all my peas, to the amount of 40 acres, at the price of 1*s.* per acre, and think that my peas are much better than thofe fown any other way. It is alfo on a very fimple plan, and the expence of it when complete is not 2*l.* It is ufed by manual labour, without any horfe; and it will draw the drill, fow the peas, and cover them at the fame time, and will fow them much rounder than any other I have yet feen. I likewife find I can do it much cheaper than with any horfe, and am of opi- Very economical drill for fowing peas.

* To whom the filver medal and ten guineas were voted by the Society. A complete machine is placed in the repofitory of the Society.

nion

Very economical drill for sowing peas.

nion that it fows much better than any drill I have feen. If the Society wifh it, I will fend a model for their infpection.

I am, SIR,

Your obedient Servant,

ROBERT GREEN.

Weftwratting, Cambridgefhire,
June 27, 1802.

SIR,

I HAVE fent the engine for fowing peas, in order that it may be laid before the Society for the Encouragement of Arts, &c. I intended to have fent a model of it, but afterwards thought that the engine itfelf would be more acceptable to the Society. I made it myfelf, and have fown with it 26 acres of land in my own occupation. Mr. Piper, a near neighbour of mine, has fown with it five acres; and Mr. Cock, of Blunt's Hall, Wratting, in Suffolk, 25 acres, at the expence of 1*s.* per acre. Several other gentlemen had drills of me for fowing peas. If I give my men 1*s* 6*d*. per acre, they will fow for me two acres in one day. I can with my own hand fow one acre in five hours, and at the fame time fow the peas, draw the drill, and cover them, and make full twelve drills and a half to the rod. I likewife produce the plant much handfomer than any other feen in our country, and at a very trifling expence. By this too, the labour of horfes is fpared, which we find to be a very material circumftance. It will be a moft excellent engine for gardeners in the neighbourhood of London; for I will be bold to fay, that no man can fow with his hand, fo as to equal this, at a very trifling expence.

I have fpent much time in making implements of hufbandry, but have made none fo ufeful as this; for it is fimple in its conftruction, may be purchafed by any man, the expence being fo trifling, and faves the labour of horfes.

I remain, Sir,

Your moft obedient Servant,

Weftwratting. ROBERT GREEN.

C

Defcription

Deſcription of the Engraving of Mr. Robert Green's *Hand-Drill, for ſowing Peas, Beans, &c.* Plate II.

Very economical drill for ſowing peas.

Fig. 3. *a a*, The wheels placed upon a wooden axis *b*, which is ſquare at each end, but round in the centre. The ſquare ends of the axle have holes throughout them, at different diſtances in order to depoſit the ſeed at nearer or more diſtant intervals, as may be wanted.

c, The box in which the ſeed is placed: the axis *b* is cylindrical, and has holes made therein proper to receive the ſeeds, which by the revolution of the axis are carried forwards, and fall through an iron tube into the ground opened for them by the ſhare *d*. When depoſited in the ground, they are covered, or the earth drawn over them by two iron pins or ſcrapers *e*, fixed on each ſide of the tube, and extending ſome inches behind it.

f f, The handles of the drill-machine, by which it is puſhed forwards.

Fig. 4. Shows an enlarged view of the interior of the ſeed-box *c*, above mentioned, and holes for the ſeeds placed in a ſpiral line, in order to drop the ſeeds more regularly.

g, Is a ſmall bruſh within the box, which rubs againſt the cylinder, to keep the holes clear to receive the ſeeds.

Fig. 5, Is a ſection of the machine, where *a* is part of the ſeed-box; *b*, the round part of the axle, which delivers the ſeed.

d, The ſhare which opens the earth.

h, The tube through which the ſeed falls.

i, The mouth of the tube, and one of the fins which draws together the ſoil, and covers the ſeed.

k, Is a ſmall door, to be opened occaſionally, if the roller or tube are out of order.

l, A ſtrong flat board, to which the iron work is ſcrewed.

Fig. 6, Shows an enlarged plan of the iron work, when the machine is reverſed.

d, Is the ſhare.

i, The hole from which the ſeed is dropped.

e e, The two fins, or ſcrapers, which collect the earth and cover the ſeed.

N. B.

N. B. The length of the upper rim of the feed-box of the machine in *Fig.* 3, being fifteen inches, will serve as a standard for the measure of the other parts *.

V.

Enquiries concerning the Methods of investigating the Course and Velocities of a Body supposed to be projected from the Moon to the Earth. By a Correspondent.

To Mr. NICHOLSON.

SIR,

Theory of La Place, that stones fall from the moon.

AMONG the various theories concerning the stones fallen upon the earth in different parts of our globe, (the subjects of Mr. Howard's Analysis), that which De la Place has ventured upon, though apparently incapable of proof, seems the least improbable, namely, that they are projections from the lunar volcanos. Surely, Sir, whether this is or is not *possible*, may be mathematically demonstrated, certain data being allowed. If you, or any other able mathematician, could find time to do this, it would be a great gratification to see the solution of these questions.

Data for computation.

We must assume as data, That the density of the materials composing our satellite, is precisely similar to the density of our globe; and that the moon has no atmosphere to resist the projection from its surface, or so small a one as not to be calculated upon, since, probably, it is rare and low, perhaps not more than one-sixth of a mile high.

Now their relative bulks and distances are *pretty well* ascertained, and we will take them to be *precisely* known.

Their comparative centrifugal forces may easily be calculated, as the moon revolves about 27 times slower about its axis than the earth about hers.

These points must be settled, because particularly the last must have considerable effect in determining the line such projected body would describe.

* This account was also supported by a certificate from eight persons who had used the drill in sowing 113 acres of land, and of eleven farmers who witnessed and approved its operation.

Let

Let us suppose too, that this body weighs one hundred weight.

What will be the velocity requisite to overcome the mutual attractions of the moon and this body, so as to project it beyond their powers; remembering that its velocity will be continually diminishing as long as any attracting power acts upon the projectile, and calculating the aid it would receive from the centrifugal force. Results to be investigated.

In the journey of 240,000 miles through which it has to travel in a direct line, is there any free space beyond the sphere of the moon's attraction and that of our planet's? If so, at what distance from the moon will that be found? and at what distance from the earth? and with what velocity may it be supposed to travel through that space? of course it will be much slower than when first projected.

As upon entering the limits of the earth's attraction its velocity will be again increased, *quære* its rate of travelling to the earth; and, taking the three reckonings into account, in how many days and hours can it be mathematically demonstrated that it would reach our solid globe?

Acted upon by the united forces of projection, centrifuge, (if I may coin a word) and the motion of the moon in its orbit, and the force of attraction and orbital motion of the earth, what will be the precise line it may be presumed to describe in its course?

Would it not have a revolving motion during some part of its course?

What are the calculations by which we may be enabled to judge that 3 or 5 times the velocity of a cannon-ball, at the moment of projection, would enable it to counteract these impediments?

It has surprized me, that the numerous late publications mentioning this theory, have not detailed the mathematic processes by which it seems capable of being solved, or of proving its fallacy.

I hope, Sir, you will not think this obtrusion impertinent. You obligingly and satisfactorily complied with my request in a note concerning Col. Blaquiere's gun to throw double headed shot; this has emboldened me to express my wishes on the subject of this letter. But I do not wish to have my ignorant questions

load

load your valuable Journal, though I ſhall feel myſelf much obliged by any further information upon this head.

I remain, Sir,

Your moſt humble ſervant,

A CONSTANT READER.

VI.

Drawing and Deſcription of a Three-Furrow Plough. By the Rev. EDMUND CARTWRIGHT, *of Woburn, Bedfordſhire.**

To CHARLES TAYLOR, Eſq.

DEAR SIR,

Economical three-furrow plough.

I ENCLOSE you a certificate of the performance of a plough of my invention, which has occaſionally been at work through the whole ſummer. For this laſt fortnight, it has been uſed for ploughing in wheat under furrow. Though a very uſeful inſtrument at all times, it is particularly valuable at the ſeed times, and the turnip ſeaſon; becauſe at thoſe times it frequently happens you loſe the moſt favourable opportunities, for want of ability to execute your operations with ſufficient diſpatch.

Weekly ſaving 4l. 16s.

I need not calculate to you the ſaving on the uſe of this plough. It is worked (on light land I mean) with a pair of horſes, without a driver. A pair of horſes and a ploughman cannot be laid at leſs than 8s. per day. As two ſets of theſe are ſaved, the weekly ſaving by the uſe of this plough amounts to no leſs than 4*l.* 16*s.*

Applicable to level lands in a tolerable ſtate of cultivation.

Uſeful, however, as I find this inſtrument on our light level lands, I am not ſo partial to it, to ſuppoſe it is equally calculated for all ſoils, or all kinds of ground. For inſtance, where the ground is very uneven, or the ridges are narrow and ſteep, I would not uſe it; neither when the land is very foul with root weeds. In all theſe caſes a ſingle plough is certainly to be preferred: but in all caſes where the ground is in a tolerable ſtate of cultivation, and where it lies reaſonably level, it will be found a moſt valuable acquiſition.

* From the Tranſactions of the Society of Arts, who voted him the ſilver medal. A model is placed in their repoſitory.

I will

I will thank you to communicate this letter, and the certificate accompanying it, to the Committee of Agriculture; and and if they are disposed to think favourably of this invention, I will send you a model for their inspection.

I am, DEAR SIR,

Your very obedient servant,

EDMUND CARTWRIGHT.

Woburn, Oct. 20, 1802.

This is to certify, that the three-furrow plough invented by the Rev. Edmund Cartwright, ploughs a surface of twenty-seven inches each bout, and that on light land a pair of horses regularly ploughs three acres per day with it in a workmanlike manner. Certificate.

JOHN DUCKITT, *as Bailiff to his Grace the Duke of Bedford.*

WILLIAM BAXTER, *Assistant.*

June 21*st*, 1802.

DEAR SIR,

YOU herewith receive the model of my three-furrow plough. Why this plough saves power.

The saving of hands, and consequently of expence, in a plough of this kind, is obvious; but why there should be a saving of power, may require to be explained.

I need not observe to you, nor to any man who considers the action of a common plough, that a very material part of the labour in ploughing, arises from the friction of *the land side* and *the sole;* of the one against the side of the furrow, of the other against the bottom. In a single plough a certain length and width are required in those parts of it, to make it go steady; and even then the effect would be imperfectly obtained, did not the ploughman assist by the leverage of the handles of the plough. Hence it is clear, that the less disposition any plough has to follow the draught in a strait line, the greater is the labour of working it, because the ploughman in that case is to exert a greater power of leverage to keep it steady. On the contrary, when two, three, or more ploughs are combined, they serve to steady each other, and require comparatively very little power of the lever to keep them in a strait line. Under these circumstances,

Economical three-furrow plough.

circumstances, neither the first nor second plough has any sole or land-side whatever; and even the third does not require so much of either as a single plough. I calculate the saving of power from the consideration alone, as equal at least to one plough. What farther power is saved, I attribute to the lightness and compactness of the instrument.

I am willing to think the simplicity of its construction, and the manner of fixing the plough (consisting but of two parts) to the beam, will not escape your observation. When the cutter (for as it is both coulter and share, I can give to it no other single name) requires to be sharpened, or new-laid with steel, by drawing the two bolts the whole is set at liberty.

I make the ploughs to fit each beam indiscriminately; because when the land is too strong, or too foul, to work the three, I take off the second plough, and transfer the third into its place.

You will observe the centre of the whiple-tree shifts. By this contrivance, the power of the horses is equalized, though they may be unequal in strength, the longer lever being given to the weaker horse.

Should the Society wish for any farther information, it will give me pleasure to furnish them with it.

I am, DEAR SIR,

Very truly and sincerely, yours,

EDMUND CATWRIGHT.

Woburn, December 14, 1802.

Charles Taylor, Esq.

Reference to the Engraving of the Rev. EDMUND CARTWRIGHT'S *Three-Furrow Plough.*—Plate II. Fig. 1, 2.

Description of Mr. Cartwright's three-furrow plough.

Fig. 1. A B, the two wheels of the plough, the wheel B being full one-seventh in diameter larger than the wheel A.

C D E, the three beams of the plough, of which C is the shortest and E the longest: these beams are fixed in the strong cross piece F, at equal distances from each other, and braced by another cross piece from C to E.

G H I, the three cutters which answer the purpose of both coulter and mould-board, each being formed together, or made of one piece of beaten iron. Each cutter is screwed to its beam by the flanging-iron K.

LM, the two handles of the plough, the lower extremities of which are fixed in the two outer beams C E, and connected by a crofs piece N, to make them firmer. The handle L is longer than the handle M, in the fame proportion as the beam C is fhorter than the beam E.

Defcription of Mr. Cartwright's three-furrow plough.

O P, two upright pieces of iron fixed in the crofs piece F, having two holes at their fummits for the reins to pafs through which guide the horfes.

S, an iron bar which flides up and down near one end of the crofs piece F, to raife or lower the wheel A.

Fig. 2. Shows a detached portion of the ftrong crofs piece F, to explain the manner in which the whiple-tree fhifts (R) are fixed in front of that crofs piece, fo as to regulate or equalize the power of the horfes.

S, a bar of iron, the lower part of which forms the axis of the wheel A, the upper part flides in a groove, in the crofs piece, F, and has holes at different diftances. It may be retained at any height by an iron pin T, which paffes through the crofs piece, and one of the holes of the iron bar. The real plough is nine feet long to the extremity of the handles and each cutter turns a nine-inch furrow; from centre to centre of the beams, being nine inches.

VII.

On the State of Science among the earlier Nations of Antiquity: and more efpecially of thofe Refearches which conftitute the Subjects of Alchemy. In a Letter from E. P.

To Mr. NICHOLSON.

Rofcommon, April 1, 1804.

SIR,

IT is an authenticated fact, that much of our late fcientific acquifition, and many of thofe facts which the experimental genius of the prefent age is daily bringing to light as original difcoveries, were well known in more ancient periods of the world; and there is abundant reafon for fuppofing that, in chemiftry and metallurgy, the philofophers of thofe ages were fuperior to thofe of the prefent day.

Reafons for thinking that the modern fciences were known to antiquity.

But

But we muſt mount up much higher than what are called the dark and barbarous ages of modern Europe, or even of any of thoſe revolutions in the Eaſt of which hiſtory has tranſmitted any detailed accounts.—Science had *began to decline previous* to the earlieſt hiſtoric relation which is extant, and there appears ſufficient evidence that the Greeks and Egyptians, in their hieroglyphics, their allegoric devices, and in their mythologic myſteries, which they had blindly received from their enlightened predeceſſors, were recording for poſterity a ſeries of phyſics, of which they were ignorant, and which is now gradually unfolding.

Tranſmutation of metals.

The univerſal rage for penetrating into the ſcience of alchemy, not only indicates the ſcarcity and value of gold in all ages, but evinces, I think, that there has always exiſted ſome *tradition* of ſuch a tranſmutation having been once effected.

Decompoſition of water.

The decompoſition of water into different gaſes was certainly once known; and our recovery of that ſublime phenomenon, which ſeems the key to the great laboratory of nature, bids fair to reſtore to mankind the moſt important facts which have lain in obſcurity for ſo many centuries.

Alchemy.

Of theſe, alchemy will probably be one; it has deeply excited the attention of ſome excellent chemiſts in this iſland, with whom I have the honour of being connected: of any progreſs we may hereafter make, you ſhall be immediately appriſed; and if you, or any of your ingenious correſpondents are engaged in a ſimilar courſe of experiments, we might mutually aſſiſt, and abridge each others labours.

I have the honour to be,

Your ſincere friend and zealous well-wiſher,

E. P.

P. S. A ſeries of experiments on this ſubject will probably throw conſiderable light on the lunar (more properly lunatic) ſtones, the rational phenomena of which you have taken ſo much laudable pains to elucidate.

Deſcription

VIII.

Description of a Machine for Clearing great Roads from Mud. Communicated to the Society of Arts, by the Inventor Dr. JOHN WINTERBOTTOM, *of Newbury, Berks* *.

IN a defcription of this machine, I fhall briefly notice the five principal parts of which it is compofed; the frame, the fcraper, the chain, the fledge, and the pole; becaufe a very accurate model accompanies this paper, made upon the ufual fcale of one inch to a foot. Machine for clearing roads from mud.

The *frame* (fee *Plate* I. *Fig*. 1.) confifts of two pieces of timber AA, which at one extremity are formed into a pair of fhafts BB, and at the other are ftrongly united by three tranfverfe pieces CDE.

The *fcraper* F is placed under this frame-work, in an oblique direction, at an angle of 30°, between two of the tranfverfe pieces CD, and confequently forms an angle of 150° with the line of draught. By this pofition of the fcraper, the machine, when ufed, actually clears itfelf from the mud as faft as it is collected, and removes it into a heap on one fide, after the manner of a plough.

The *chain* G is connected with a piece of iron-work H, which projects from the lower end of the fcraper; for here additional power is required, as the whole body of the mud, which has been collected, muft pafs off by this extremity. Some advantage has alfo been gained by making this end of the fcraper fhorter than the other.

The *fledge* II is conftructed upon the upper part of the frame, that by inverting the machine it can be tranfported without injury to the fcraper, over the moft rough and ftony roads, or pavements, to thofe places where its ufe is particularly required.

The *pole* K, which is moveable, ferves the purpofe of a rudder, that when the machine happens to be forced by any great weight of mud, or folid body of earth, &c. from its proper direction, it can be eafily reftored to its former pofition: and it may alfo be obferved, that the moderate preffure

* To whom the filver medal was voted. There is a model in the Society's Repofitory.

Machine for clearing roads from mud.

of the hand upon the pole tends to make the machine fteady, and therefore caufes it to work to more advantage. In the model, the pole is made only ten inches long, inftead of fifteen, that it might occupy lefs fpace in the box. The plates in front of the fcraper, and upon the fledge, are made of caft-iron.

OPERATION:

For the operation of the machine, two men and four horfes are required: one man to drive the horfes, and another to take the management of the pole and the direction of the labour to be performed. The horfes are to be worked double, as commonly practifed, two being employed to draw by the fhafts, and two by the chain above defcribed. But the manner of ufing the machine will be beft underftood by the following fketch. *Plate* I. *Fig*. 3.

The firft progrefs of the machine marked No. 1, commencing from the arrow-mark, will remove the mud in a line to the right; the firft return, No. 2, will remove another part of it to the left. The fecond progrefs, No. 3, will take up what is left by No. 1, befides the quantity which is upon the fpace now to be paffed over, and will remove it all to the right. The fecond return, No. 4, will operate in a fimilar manner with regard to No. 2, and remove that to the left. Thus, by four lengths, more than twenty feet wide of a road can be cleared; and this has been frequently performed in the prefence of feveral perfons. The number of lengths may be increafed at pleafure, according to the width of the road.

In the neighbourhood of London, where there is inceffant travelling, it would be advifeable to ufe two machines at the fame time, one immediately following the other, as in No. 1 and 3, which will leave a fpace fufficiently wide for the largeft carriage to pafs, without difturbing the mud already fcraped up.

There is one advantage in the operation of this machine worthy of being noticed, which is, that by the ufe of it the road is made more even and fmooth, the fmall holes being filled up by the more folid parts of the mud; whereas, when roads are fcraped in the ufual way, by hand, all the irregularities are increafed, and become the future depofits of water; and it is univerfally known that thefe puddles, as they are called, are the chief caufe of the deftruction of roads.

'It has been obſerved, that ſtones are ſometimes forced up by the machine; but, it appears to be thoſe only which project in ſuch a degree as to be dangerous to the traveller, and which require to be broken for the more effectual mending of the road. Machine for clearing roads from mud.

I can ſay nothing concerning the effect of the machine upon duſty roads, having had no opportunity of trying it at that ſeaſon of the year. When, indeed, the roads are watered, as about London, there is no doubt but a great quantity of that dirt may be removed, which, in a few hours of ſcorching ſun, would again be converted into a body of duſt.

If it ſhould be objected, that the machine is too large, and that a ſmaller one, which might paſs over half the ſpace of ground that this does, and might be worked by two horſes, would be better; I muſt beg leave to anſwer, that, in my opinion, with a leſs one there would be much labour to little purpoſe; becauſe this machine, which paſſes over a ſpace of about ſix feet and a half, will not, in ſome places, when the roads are very wet and very deep, leave more than three feet clear, the mud on each ſide falling in and filling up, to a conſiderable extent, the ſpace already paſſed over: it muſt therefore be obvious, that, under ſimilar circumſtances, the track of a ſmaller one would almoſt inſtantly be obliterated.

TESTIMONIALS.

I am ſo anxious that the Society ſhould have ample ſatisfaction on this head, that I ſhould be happy if they would, before finally determining on the utility of this machine, condeſcend to make ſome inquiries in this part of the country, where it has been publicly tried.

I can however mention, with ſome pleaſure, that ſeveral gentlemen, acting as Commiſſioners of the Roads, have honoured me with their attendance during various experiments; and, having witneſſed the very powerful effects of the machine, they have given it their public approbation at the laſt monthly meeting, when the following entry was made in their minute-book:—

"At a meeting of Truſtees of the London and Bath roads, held at the Globe Inn, Newbury, on Monday, the 21ſt of February, 1803. At this meeting were preſent, James Croft, Eſq.

Machine for clearing roads from mud.

Efq. Frederick Cowflad, Efq. Rev. Thomas Beft, Mr. Richard Baily, Mr. Thomas Clark, Mr. John Baily, Mr. Jofeph Tanner, Mr. Thomas Pocock.

"Refolved, that the machine invented by Dr. Winterbottom, for fcraping off mud from turnpike roads, will be of public utility, and fave confiderable expence of labour."

After this public teftimony in its favour, I might perhaps be excufed from producing the certificates of a few individuals: it will, notwithftanding, be neceffary to give fome eftimate of the probable faving to be expected from its ufe.

In all trials made previous to the 21ft of February, the machine had been worked upon no meafured extent of ground; but the general effects were fuch, that feveral perfons of great experience in the management of roads, rated the daily work of one machine only as equal to the labour of fifty or feventy men: fifty being the loweft eftimate ever named.

A few days ago I directed fome work to be done by meafure; and I can now ftate it as the opinion of two very competent judges, that one machine will clear three miles in a day, twenty feet wide (confifting of four lengths, and making the day's work twelve miles) which is confiderably more than 120 men can do in a day.

	l.	s.	d.
120 men, at 2s. per day	12	0	0
Four horfes and two men can here be hired to work the machine for the day, at -	1	5	0
Difference - - -	10	15	0

At a diftance, where carriages run principally in the centre of the road, the chief bufinefs in the management of it confifts in keeping the fides clear and open. One machine may therefore be occafionally employed in outfide work only; that is, may go fix miles, and return, (making twelve miles, as juft mentioned) with the faving already given.

Whatever furprife thefe calculations may occafion, the Society will perhaps be fatisfied that I have not over-rated them, when I produce the refult of a fair experiment, made on the 25th of February, in the prefence of four truftees (Frederick Page, Efq. Francis Page, Efq. Mr. Thomas Clark,

Clark, and Mr. John Baily) and others, by which it appears, that two miles, by measure, on the road to Reading, were cleared from mud, to the extent of 18 or 20 feet wide, by two machines, in the space of two hours and a half, by the watch; and the work was judged to be equal to the labour of more than eighty men in a day. Machine for clearing roads from mud.

The success of this experiment was so satisfactory to the above-named trustees, for I was not present on the occasion, that they directed, without my knowledge, the remainder of our district on this road, extending seven miles, to be cleared in the same manner; and I can now declare, with some degree of pleasure, that this was actually completed by two machines in one day, viz. on the following day, the 26th of February. Of this day's work I have heard it affirmed, by an experienced surveyor, that it could not have been done in one day by 400 men.

I confess that I am myself unable, from the want of practical knowledge on this subject, to form a comparative estimate between the work done by this machine and by hand: I have therefore sought for information from persons of respectable characters, who have been surveyors, or renters of roads for many years: and I have been assured, as well by those who were present at the experiments, as by others who examined the roads afterwards, that it would require sixty men a mile, to do the work in one day, which a single machine will accomplish at four lengths; and it has been already shown, that three miles can, without difficulty, be cleared in a day: one machine will therefore do the work of one hundred and eighty men. But I have taken the average at only two thirds of this estimate, viz. at forty men per mile instead of sixty, being more willing that the power of the machine should at present be under-rated, than that the public should be deceived or disappointed concerning it.

The trustees of the London and Bath roads, being desirous of having these two machines, which had been constructed on my account, and under my own inspection, for making the experiments, I have consented to dispose of them: and as far as I am now able to judge, the price of a machine complete will be about ten guineas.

Finally, I must beg leave to advise those who are inclined to make a trial of this machine, to be careful whom they in-

 tend

Machine for clearing roads from mud.

tend to employ in the construction of it; for I can assure them, that it is not sufficient to attend only to the form of the model; but it is absolutely necessary that the different parts, and especially the two braces behind, should be firmly put together, otherwise it will be impossible for it to withstand the force that must sometimes be exerted upon it by four, or perhaps by six horses. The scraper may be made of beech or elm, &c. but the other parts ought to be made of ash; and I must particularly recommend these materials to be well seasoned; all which circumstances were minutely attended to in the two machines which were made for me by Mr. Joseph Moss, of Greenham, near Newbury.

JOHN WINTERBOTTOM.

Certificates from Mr. George Goddard, Greenham, near Newbury, Mr. Francis Page, Mr. Frederick Page, Mr. John Baily, and Mr. Thomas Clark, accompanied the above paper; stating, that on the 25th of February last, two miles had been cleared in two hours and a half, by two of Dr. Winterbottom's machines.

Reference to the Engraving of Dr. WINTERBOTTOM'S *Machine for Clearing Roads from Mud.—Plate* I.

Fig. 1, AA. Two pieces of ash timber, forming at one extremity a pair of shafts, BB.

CDE. Three transverse braces, to secure firmly the timbers above-mentioned.

F. The iron plate, or front of the scraper, fixed within the braces C D, at an angle of thirty degrees, extending on the further side two feet, and on the nearer side one foot and a half beyond the timbers.

G. An iron chain, one end of which is fastened to the outside of the timber A; the other end of the chain may be moved nearer to, or further from that end of the scraper which deposits the mud, by means of notches in the iron muzzle H, fixed to the scraper, and which regulates the draught of the horses attached to the ring at G.

K. The pole, or handle, to be made fifteen feet long, which passes through the strong holdfasts in the braces C D. This pole acts as a lever, as the scraper may be raised or sunk by

by it, at pleasure. The person who holds it may direct the scraper in its proper line, and assist it in overcoming any obstacles it may meet with in its way, or in giving it additional pressure where necessary. Machine for clearing roads from mud.

II. Show the two parts of the machine which form the feet, or sledge part of the machine, on which it slides when reversed, and which enable it to be removed from place to place, when the scraper is not in use. These feet are strongly fixed to the timbers AA, and strengthened by a transverse brace betwixt them.

L. Is the iron chain, or back band, which lies upon the cart-saddle of the horse in the shafts, and which supports the shafts.

Fig. 2. Shows, on an enlarged scale, the iron-work, fixed on the outside of the shafts, to which the chain and horse are attached.

Fig. 3. Describes, in a small extent, the track usually made by the scraper in a large way, in four rows, commencing at the arrow mark, in the track No. 1, returning after it has gone any length required by the track No. 2, proceeding again by the track No. 3, and forcing the mud collected by the tracks No. 1 and 3 to the right side of the road, and, on its return by the track No. 4, depositing the mud of the tracks No. 2 and 4 on the left of the road, as is more fully described in the preceding account, and thus clearing from mud a breadth of road twenty feet wide, by four passages of the machine.

IX.

Description and Drawing of an Hydraulic Machine, with an Account of several Inventions of early Date, which have been since brought forward by later Inventors. Extracted from a Foreign Work published early in the last Century. By a Correspondent.

To Mr. NICHOLSON.

SIR,

I HAVE lately met with a work in French, entitled *Recueil d'Ouvrages curieux de Mathematique et Mecanique, ou Description du Cabinet de Monf. Grollier de Serviere,* in quarto, printed Account of the publication of the Museum of Monf. de Serviere.

printed at Lyons in 1719. The book is divided into three parts, the first of which contains curious engravings of delicate, eccentric, swash and rose-work turnery, the methods of producing which are to be found in Moxon's Mechanic Exercises, and in other later works on that subject. Upon this part I shall make no other remark, than by demanding of your correspondents, whether the oval chuck, or engine for turning ovals, was unknown so late as the period above specified. If Mons. de Serviere had known it, I should suppose he would have introduced ellipses among the various figures he has exhibited. The second part of the work consists of clocks or time-pieces, more remarkable for some singularity in figure or structure, than any improvement in the art of measuring time; and, the third part contains models of hydraulic machines, with some engines for military and other purposes. Many of the hydraulic machines appear better calculated to be shewn in a model, than carried into effect on a larger scale, and, among the other engines, I see nothing which, at the present day, would much conduce to the entertainment of your readers. I have, therefore, sent you a drawing of the hydraulic engine exhibited in his 49th plate, and after the description, I will mention a few other objects which have been thought of more modern invention.

Turnery.

Time-pieces.

Hydraulic machines.

Hydraulic machine described. It consists of two pinion working in an elliptical box.

In *Plate* IV. *Fig.* 2. A and B represent two solid pinions made in wood or metal, and occupying all the interior space of the oval box or chamber C D; in which they turn freely and take into each other. The chamber C D is to be well and solidly made, having an opening below at D, as in the figure, and also at E, where the aperture corresponds with the bore of a pipe F, applied and fixed to the same. Every other part is well closed and secured. *Fig.* 3, represents the cover or cap.

This chamber is properly fixed under the water of the well, or cistern, out of which the supply is to be obtained, and in this situation, the elbow or handle G, *Fig.* 1. *Plate* IV. is fixed on the square of the axis A. This handle is connected with another H, by the iron sliding piece I, which moves upon the fixed pin K, and obliges its two extremities constantly to move alike. Whenever therefore the handle H is turned by the first mover M L, the other arm G must also revolve together with its pinion, and by consequence the other pinion B.

When

When the two pinions turn (so that their upper teeth may constantly approach each other, while the lower recede,) the water which lies between their teeth in the lower part D of the chamber, will be carried round till it arrives at the part C, where it will be compressed by the continual augmentation of water, which is brought thither between the teeth, (or rather by the perpetual diminution of the space between the two uppermost teeth that touch the chamber). So that the fluid will enter the pipe F, and be forced to the intended place. Its action.

Thus far I have translated from Monf.r de Serviere. The machine appears to be ingenious, and though liable to the objections of wear which you very properly urged against the schemes of O. B. and others *, seems preferable to them in several respects. I think it would be an improvement, to cause the axis A and B to drive by a connection of external wheel-work, instead of depending upon their interior teeth, which require to be well figured and fitted, and kept so, by not loading them with pressure of one surface against the other. It seems scarcely necessary to remark, that we have better methods of connection for work at a distance in our cotton gear and elsewhere, than the sliding piece I; and lastly I would propose it as a mathematical exercise to determine the law of the velocity of the fluid through F, when the rotation of the machinery is uniform. Remarks on the engine.

Among the machines which I believe have been considered as of later date, but are found in this work, are the engine mentioned in Desaguliers, for raising water by a losing and gaining bucket, and regulated by a fly; the chain pump, for which Cole had a patent, but which is known to have been of very ancient use in the Chinese Empire; the horse mill worked by the wheels of a carriage, as lately proposed by the ingenious Adam Walker; and the gouty chair of Merlin, worked by two small handles connected with a pair of small wheels attached to the fore feet. Old engines re-invented. Losing and gaining bucket. Chain pump. Adam Walker's horse mill.

I am, Sir, with esteem,

Your obliged Reader,

R. B.

* Philos. Journal. Quarto series. IV. 468.

X.

An Examination of Dr. Wollaston's Experiment on his Periscopic Spectacles. By Mr. William Jones, *F. Am. P. S.*

To Mr. NICHOLSON.

SIR,

Preliminary observations.

THE inferences that Dr. Wollaston has thought it best to publish in your last month's Journal, instead of a direct reply to my refutation of his new principle of spectacle glasses, are of themselves sufficient to convince any impartial person of the validity of the objections advanced by me in your preceding Journal; and, notwithstanding an extraordinary experiment he has therein related, as made only by himself, I should not have thought it requisite to trouble your readers again, but for the unfounded imputation that he has declared against me, that of having by an experiment deceived myself. I trust, Sir, I may be allowed, in contradiction to this, to observe, that, after more than 20 years experience in the practice of my profession, such as daily administering to decayed vision, and employment in the construction of all kinds of optical instruments, I should not be acquainted with all the various properties of lenses, singly or combined, and especially of so simple and well known a form of lenses as adopted by him, is an idea, that I am confident he will not be able to impress upon the minds of the public. I suggested no new experiment, nor was any one wanting; the definitive laws I adduced, were contained in the works of the best writers on optics, and were sufficient to evince the want of originality and improvement of his meniscus shaped lens. In respect to the experiment by which he attempts to inforce a proof of an advantage in his spectacles, its value will be known by the following account of a repetition of it.

Experiment with a pair of periscopic spectacles compared with a pair of the usual construction.

I am possessed of a pair of his periscopic glasses, mounted in a single steel frame, which cost 10*s*. 6*d*. The glasses, I must observe, are different to his proposed form, having of each, the inner side, or that next to the eye so little incurvated, that by any person but an optician, they would be called plano convexes. The focus is four inches, the same as used by Dr. Wollaston in his experiment. In a similar mounting, with

with double convex glasses of the same diameter and focus, I provided a pair of our own manufacture, and as sold by us at 3s. 6d. These two pair of spectacles were attentively compared together, by myself and several judicious and impartial persons, in the manner as stated by Dr. Wollaston of his. The result was as follows.

Result. With the double convex lenses. Extent of distinct vision. Indistinct.

The convex glasses being applied as close as possible to the eyes, with the frame attached to the head, the print of a large quarto page was viewed through them, at a distance for distinct vision at their centres, the letters at the distance of about 25 lines, appeared quite distinct or well defined; giving the axis of the eyes a little obliquity to discriminate more lines, an indistinctness or confusion of letters commenced, increasing towards the extremity of sight, and from the lateral aberration of the lenses, the letters were tinged with the prismatic colours. Keeping the head fixed in the same position, the periscopic glasses were substituted. The extent of distinct letters without distortion was nearly as great, but the coloured letters were evidently nearer to the centre, and more numerous than by the other glasses. By inclining the axis of the eyes still more than in the former case, or looking extremely asquint through the glasses, a greater extent of lines was observed, but blended with colour and confusion. The optic nerves felt a sensible irritation, evidently from the squinting position of the eyes, a refraction of many superfluous rays, and the consequent increased and mutual magnitude of the images on the retina. The pain in the eyes mentioned by Dr. Wollaston, must have arisen only from this circumstance, and not from the one he represented it to be. By a trial of the old meniscus glass I before mentioned, which is of four inches focus, and corresponds with what he has a patent for, in comparison with one of the above plano convexes, the view of letters was still more extended, but illegible and with much colour, and like the others towards the extremity, of no sort of use for the purposes of vision. Now all this is conformable to the laws of optics, and manifests a property different to that advanced by Dr. Wollaston.

With the periscopic glasses, more colour.

Trial of another meniscus.

These several glasses are also at the public service for inspection in our shop in Holborn.

Concluding remarks.

By making the glasses of the above periscopic spectacles nearly planos, Dr. Wollaston's principle is destroyed, and my opinion

opinion evidently verified; that the nearer a meniscus approaches to a plano, the more perfect it will be, as the spherical surface, for the same focus, is diminished, and consequently the aberration besides; admitting that there were any advantage desirable from a great obliquity of the axis of the eyes to those of the meniscus shaped spectacle glasses. I would ask, for what reason has man his head moveable? was it not, that he should place his eyes directly before the object to be viewed, and not subject himself to fallacious ideas of them, by an aukward and revolutionary squinting. From what I have advanced, I doubt not of the public decision; (from a fair comparison of the two kinds of spectacles) in favour of the established double convex spectacle-glasses, for

"*Magna est veritas et prævalebit.*"

I am, Sir,

Your respectful humble Servant,

W. JONES.

Holborn, April 10, 1804.

XI.

Experiments and Observations on the Change which the Air of the Atmosphere undergoes by Respiration, particularly with Regard to the Absorption of Nitrogen. In a Letter from ALEXANDER HENDERSON, M. D.

To Mr. NICHOLSON.

SIR,

Whether nitrogen be absorbed in respiration.

I TAKE the liberty of communicating to you a brief detail of some experiments on respiration, which were undertaken chiefly with a view to determine the absorption or non-absorption of *nitrogen*, a point which has been hitherto much controverted, but which I flatter myself to have now sufficiently ascertained.

The gasometer.

These experiments were performed by means of a gasometer, capable of containing about 2200 c. inches, and graduated so as to shew a difference of 2 c. inches. In breathing from this apparatus, the inconveniences from friction were very inconsiderable. Towards the end of the experiment, however,

ever, when the air in the gasometer became vitiated, and the respirations fuller and quicker, some disagreeable effects were experienced from the smallness of the breathing tube, and from it becoming, in some measure, choaked by the vapor from the lungs, which was condensed in it.

After closing the nostrils, and making a forced exhaustion of the air in the lungs, a full inspiration was made of the atmospherical air in the gasometer, and this inspiration as well as the subsequent expiration, was measured, by means of the graduated scale. The air of the gasometer was then respired as long as possible, and till the oppression about the chest became so great as to oblige the experimenter to desist, and before closing the stopcock, and separating the tube from the mouth, the magnitude of the last full inspiration and expiration was accurately observed.

Method of respiring,

In all our experiments, the bulk of the atmospherical air was considerably diminished by respiration, the quantity that disappeared varying from five to eight c. inches per minute. This diminution, however, is partly to be attributed to the condensation of the oxigen gas, when converted into carbonic acid gas, in which form its bulk is diminished about one fourth. In order to obviate any objections that might apparently arise from the difference of the inspirations and expirations at the commencement, and at the conclusion of the experiments, they were, as we have already mentioned, carefully noted; and if any difference did occur, it was deducted on the side where the preponderance took place.

and general effect, as noted;

In proceeding to examine the chemical qualities of the air, considerable difficulties were at first experienced for want of an accurate eudiometer. Several trials were made with *nitrous gas*, which affords the most easy and expeditious mode of analysing atmospherical air. But, independent of the inaccuracies which may arise from the number of vessels it is necessary to have recourse to in employing it, this test is liable to still greater objections, from differences in its degree of purity, from its absorbing a portion of nitrogen, and from its combining with a greater or less proportion of oxigen, according to the diameter of the vessels employed, the length of time which it is allowed to remain in contact with atmospherical air, and the degree of agitation used in mixing them.

Eudiometrical examination.

Nitrous gas exceptionable.

The

Seguin's eudiometer with phosphorus objected to;

The eudiometer with *phosphorus*, as recommended by Seguin, is by no means free from these disadvantages. It is subject to the same inconveniences as the nitrous gas, with respect to the frequent change of vessels; and, if we may credit the observations of V. Humbold, is liable to several sources of fallacy from the different proportions of oxigen, which the phosphorus absorbs, and from its combining also with nitrogen. A portion of carbonic acid may also be generated, if the phosphorus be not perfectly free from impurities. The eudiometrical test, which has been lately recommended by Davy, viz.

and also the impregnated sulphate of Davy.

a solution of the *pale sulphate of iron*, saturated with nitrous gas, is not altogether exempt from the inconveniences which attend the use of the latter substance, and has the additional objection, that a portion of nitrogen is generally disengaged from the solution, after the absorption of oxigen is completed.

Sulphuret of alkali or lime absorb oxigen very slowly,

The *sulphurets of alkalies* and *lime* have hitherto been regarded as among the most accurate tests of the purity of atmospherical air. They, however, absorb oxigen but slowly, and several days may elapse, before the absorption be completed. It is therefore, in general, necessary to have recourse to complicated and uncertain calculations, in order to adjust the result of the experiment to the variations of the surrounding

and are not completely amended in this respect by heat.

atmosphere. To remedy this objection to their use, the application of heat was proposed by Guyton, by which means a speedy absorption was effected; but, besides, that some difference of the product may result from the method employed, the apparatus itself is objectionable, from its size and inequality of dimensions.

The apparatus of Dr. Hope affords great advantage,

Luckily for the progress of eudiometrical science, these obstacles to the attainment of an accurate knowledge of the constituents of the atmosphere, seem to be now, in a great measure, removed, by the invention of an apparatus by Dr. Hope, which unites the advantages of neatness and simplicity with those of extreme accuracy and expedition, and of which you have already given a very full description in your Journal *. In the trials which I have made with this instrument, the substance found to answer best as a test of the

Sulphuret of lime was used.

atmospherical air was the *sulphuret of lime*, which is more

* Vol. VI. pages 61. 210.

readily

readily decompofed than the fulphuret of potafh, and is confequently better adapted for the purpofe of expedition. In moft of the experiments, the abforption was finifhed in about twenty minutes, if fufficient agitation had been employed. The only objection, to which the above teft may feem liable, is, that, according to fome, it abforbs a fmall proportion of the nitrogen, as well as the oxigen of the atmofphere. This opinion, however, does not appear to be well founded, for in feveral of our experiments, where the quantity of oxigen abforbed appeared to be unufually fmall, and where the agitation was continued for a much longer time than neceffary, no perceptible alteration in the bulk of the air was obferved *.

It does not appear to abforb nitrogen.

The method of proceeding in the analyfis of the air, was briefly as follows: after having afcertained the purity of the atmofpherical air by means of the eudiometer above-mentioned, and knowing the exact bulk of the air contained in the gafometer, the total quantities of oxigen and hidrogen in it were calculated by a very fimple procefs. This air was then refpired, and its diminution marked as has been already defcribed. After refpiration, a portion of it was introduced into the eudiometer, and its carbonic acid was abforbed by means of lime water, (for which the above-mentioned inftrument was found extremely convenient). Freed from the carbonic acid, the air was now fubjected to the action of the fulphuret of lime, and the relative quantity of nitrogen contained in it was thus difcovered. Then, by deducting the quantity of carbonic acid, and of oxigen gas, contained in the air of refpiration, from the total quantity that remained after refpiration, we procured the proportion of nitrogen, which abftracted from the total quantity before refpiration, gave the proportion of nitrogen abforbed. Thus, let a reprefent the original quantity of nitrogen; b the carbonic acid, and c the oxigen of the air of refpiration, and M the bulk of the refidual air; $M-b+c=n$, or refidual nitrogen, and

$$a-n=x \text{ of nitrogen abforbed.}$$

The procefs. Previous examination of the air,

and after refpiration.

Experiment I. June 16, 1803.

600 c. inches of atmofpherical air were refpired, for four minutes, at the temperature of 63°.

Exp. 1, 2, 3. The bulk of nitrogen abforbed in refpiration was about 1-40th of the whole, or

* De Marti is of opinion that the hidrogenated fulphurets abforb nitrogen only when recently formed. Vide Journal de Phyfique, Tom. LIII. p. 176.

Before

about 1-32d of the nitrogen.

Before respiration, 100 parts contained of

Oxigen	.22
Nitrogen	.78

After respiration, quantity diminished to 570 c. inches, 100 parts found to contain of

Carbonic acid	.07
Oxigen	.14
Nitrogen	.79

570—39.7+80=450.3.

468—450.3=17.7 c. inches of quantity of nitrogen absorbed.

Experiment II. June 18, 1803.

600 c. inches atmospherical air were respired, for four minutes, at the temperature of 64°.

Before respiration, 100 parts contained of

Oxigen	.22
Nitrogen	.78

After respiration, quantity diminished to 570 c. inches, 100 parts found to consist of

Carbonic acid	.08
Oxigen	.12
Nitrogen	.80

Quantity of nitrogen absorbed, therefore, =12 c. inches.

Experiment III. February 11, 1804.

1000 c. inches atmospherical air were respired for the space of 4½ minutes, at the temperature of 57°. Barometer =28,78.

Before respiration, 100 parts contained

Oxigen	.22
Nitrogen	.78

After respiration, quantity diminished to 962 c. inches.

After respiration, 100 parts contained

Carbonic acid	.07½
Oxigen	.13
Nitrogen	.79½

Quantity of nitrogen absorbed, therefore, =15.1 c. inches.

Comparative remarks on these experiments and those of Davy.

These different experiments, (which I have selected from among many others) agree in the general result, viz. that a portion of nitrogen is abstracted from the atmospherical air, by the blood in its passage through the lungs, although the amount

amount is somewhat less than has been stated by Davy, who makes it equal to about 5 c. inches per minute. Yet I am inclined to think, that this is more a difference in appearance than reality; for if we consider that the most of Davy's experiments give the result of the changes produced on the air by a single inspiration, or by a small number of respirations, while in the experiments just described, a large portion of air was breathed for a considerable length of time, so as to become, at last, unfit for the due performance of respiration; it is probable that the blood could no longer produce the same alterations in its properties, that took place when a purer atmosphere was inspired. It is also natural to suppose, that the quantity of air consumed in respiration varies in different persons, and in the same person at different times. An approximation to the truth, therefore, is all that we can expect to obtain in the determination of this question; and we must rest satisfied with the knowledge of the important fact, that nitrogen is absorbed by the human body in respiration.

The striking uniformity in the analysis of the atmospherical air, by means of the sulphuret of lime, occurred also, as I have with pleasure observed, to Dr. Thompson in the numerous trials which he has made on this subject; and furnishes a most interesting problem for the investigation of the chemist and natural philosopher.

Dr. Thompson also found sulphuret of lime a very excellent test.

I am, with much Respect,

SIR,

Your most obedient Servant,

AL. HENDERSON, M. D.

Edinburgh,
April 12, 1804.

Observations

XII.

Observations and Experiments tending to ascertain the Causes of those Irregularities in Chronometers, which are generated during considerable Intervals of Time, and have been ascribed to external Causes. By Mr. JOHN HALEY, *Jun.*

To Mr. NICHOLSON.

SIR,

HAVING seen in the last number of your very useful Journal some remarks on chronometry, I have thought the accompanying observations might not be unacceptable. If you should consider them worthy of insertion, I shall take the liberty of communicating the results of some experiments I am now making in the same line.

I am, SIR,

Your most obedient and humble Servant,

JOHN HALEY.

March 29, 1801. *25, Cleveland Street, Fitzroy Square.*

Latent causes of error in time-pieces.

MY design in writing the following pages is to develope certain latent causes of error, and their mode of operation, which have been found in a greater or less degree to affect every machine hitherto constructed for measuring time.

If the errors of the machine be ascribed to foreign causes, improvements will cease.

It is certainly the part of a watch-maker to search for and remove every mechanical cause that may affect the going of his machines, before he hastily concludes that the causes of those errors which are generated in a length of time, (and which have escaped his notice,) are foreign to its structure; such as the influence of the oil, the varying different density of the atmosphere in which the wheels move, &c. For such opinions when once adopted, must greatly tend to slacken his pursuits and put a stop to his hope of farther improvement.

The foreign causes are of little effect: For many machines have performed wonderfully well, while subject to their action.

That these foreign causes do in fact produce errors deserving much notice in the best adjusted time-keepers, is probably an erroneous notion. For if so, how could so many of different constructions have been found to perform for very long periods of time, exposed to all these causes, with a wonderful degree of

of accuracy. Indeed Mr. Cumming, in his treatiſe on clock and watch work, has ſuppoſed that the influence of the oil may be in ſome caſes even beneficial to the performance of the machine; though it muſt be allowed he there ſpeaks of ſuch as have no proviſion againſt the effects of heat and cold. And though Mr. Mudge in ſome of his letters to his Excellency the Count de Bruhl does write, that when the air was moiſt in Devonſhire (where he made and tried his machines) they retarded their rates; yet the quantity of error produced by this ſuppoſed cauſe was ſo ſmall, that it gave him no uneaſineſs with regard to their ſate. But the very different performance of machines on the ſame conſtruction, ſtrongly induce me to believe that moſt of the errors hitherto found in the beſt time-keepers have been produced by latent mechanical cauſes coexiſtent with the machines, or rather, with the times they were firſt put in motion.

Opinions reſpecting the cauſes of irregularities. Variation in the maintaining power, inconſiderable.

Various opinions have been entertained by gentlemen of ſcientific and mechanical acquirements, and by artiſts upon this ſubject. One in particular has very generally prevailed, namely, that the errors have ariſen from inequality of power derived from the main ſpring, and the train of wheels. That very great errors will be produced by theſe cauſes, (if not removed by good workmanſhip,) muſt on all hands be admitted; but where the execution has been correct, the errors will be trifling indeed, and muſt always remain nearly the ſame. The late Mr. Arnold, on being aſked by a Committee of the Houſe of Commons his opinion of the Remontoire, ſaid, that it was only a help to bad workmanſhip.

Remontoire condemned by Arnold, but eſteemed by Harriſon.

Mr. Harriſon's opinion of the Remontoire was different from that of Mr. Arnold; for he expected to arrive at perfection in his machine from introducing it, though by his method of application, he did not detach his eſcapement from the whole of the train of wheels. The invention was undoubtedly a great proof of his ſuperior ingenuity, and merited high praiſe. However, on finding the going of his watch not to anſwer his expectation, he attributed its irregularity to the thermometer not having its due effect; and aſſerted "that if it could properly be put into the balance, the watch would go within a few ſeconds a year:" which aſſertion has ſince been proved erroneous.

Mr.

Mudge's Remontoire for winding up every vibration.

Mr. Mudge's comprehensive mind suggested a Remontoire that should be wound up at every vibration of the balance, and he attached it to a 'scapement which was certainly very superior to Mr. Harrison's, and for uniformity of excellence in its going, is more to be depended upon, in my opinion, than any other which has hitherto been applied to a portable machine. Mr. Mudge in the construction of this 'scapement, had almost rendered his Remontoire useless with respect to one purpose for which he introduced it. His son, in a book entitled a Narration of Facts, (pages 46 and 47, in the margin) has published an extract from a manuscript of his father's, wherein he writes as follows, "was I to make a watch myself upon this reasoning, I should not expect it to be by any means perfect, but I cannot help thinking, that I should in the first essay get rid of so many, and so great errors, that the cause of those that remain would be more comeatable than when blended as they are now, &c." I say, that he had, in the formation of his admirable 'scapement, nearly annihilated those errors, the cause of which he proposed to arrive at the knowledge of by the introduction of the Remontoire; it is much to be lamented that this great mechanic's powers failed him through age and infirmities, at the time when he had almost arrived at the knowledge of those causes of error which he so earnestly sought after. And we may presume from the superiority of his penetration, that he would also have accomplished the means of removing them, and consequently of giving to his machines all the perfection of which their principles were susceptible.

He supposed it would detect unknown causes of error.

Mudge's watches accelerated their rates at first.

It is observable that Mr. Mudge's watches, (which I consider from their construction as having the causes of error which I shall endeavour to develope, operating in them in a less degree than any other time-keepers,) in almost every instance accelerated their rates of going, while under trial at the Royal Observatory. The Rev. Doctor Maskelyne has, I think, done mechanics an essential service, by publishing his very judicious and accurate remarks upon their going while under his care. He observes, among other facts, "that the watches did generally accelerate their rates of going with sometimes retarding them a little, that they accelerated their rates, less in the second trial than in the first, and least of all in the last trial, and that towards the latter part of that trial the

Dr. Maskelyne's account of them.

the watch *green* retarded its rate." If we carry our obfervations farther, we find, that, in their fubfequent trial which took place at Mr. Dutton's, (the watches having had nothing done to them,) they both retarded their rates throughout. I will endeavour to fhew why thefe watches did accelerate and retard their rates in this manner.

The errors of oppofite tendency that did exift in thefe machines were fo fmall, and I may add, were fuch a length of time in generating, that it became very difficult for Mr. Mudge to afcertain their caufe. Where was he to look for them? It was not to the 'fcapement only his fearch was to be confined, but he had alfo to traverfe through the whole train of wheels: For though he had applied his Remontoires beyond the train, yet the laft wheel (if there was any inequality in the power derived from the main fpring or train) would prefs fometimes ftronger, and at others weaker upon the pallets of the Remontoires; which pallets the balance in its vibrations (by means of the pins in the crank affixed to it) had alternately to unlock, before it could be impelled by the unbending of the Remontoire fprings—therefore, as he fuppofed the caufes were not determined to the 'fcapement, he knew not where to fix them *.

Great difficulty of afcertaining the caufes of error in Mudge's time-pieces, if in the train.

It was an opinion (in writing) of Mr. Mudge's, " that the fimple principles of all watches are the fame and perfect, the errors found in them are therefore not errors arifing from the principles, but from imperfections, infeparable from all mechanic operations."

Opinions of mechanics. 1. Of *Mudge*, that time-pieces err not from principle, but imperfect execution.

The late Mr. Arnold afferted before a Committee of the Houfe of Commons, that, though he put no oil to his 'fcapement, yet it wore lefs than any other part that was fubject to wear.

2. *Arnold:* that his 'fcapement wore lefs than other parts.

Mr. Emery told the fame Committee, that the 'fcapement which he made for his Excellency the Count de Bruhl, after a model of Mr. Mudge's, was very difficult to execute, but when made, it would not eafily wear out. It is a known fact, that this watch went with an unufual degree of excellence.

3. *Emery:* that *Mudge's* 'fcapement would not eafily wear.

* For drawings and defcriptions of Mr. Mudge's 'fcapement, the detached 'fcapement now in general ufe, and feveral others, fee Philof. Journal, quarto feries II. p. 49.

4. General observation; that when the 'scapement wears there is no more regularity.

And ſtill more it is the obſervation of every watch-maker, when ſpeaking of the verge and horizontal 'ſcapements, that when once the verge or cylinder begins to wear, there is an end of all good performance, and that the going of the watch becomes from that period totally incorrect.

Inference: that the errors in the beſt time-pieces are thus cauſed.

Theſe ſeveral teſtimonies tend to induce a belief, that the errors which ſtill remain in the beſt time-keepers, are produced chiefly by this cauſe; and this cauſe muſt exiſt from the moment the machine is firſt put in motion, although the errors will not manifeſt themſelves to any conſiderable extent, (particularly if the materials are of the beſt kind,) until the machine has been going for ſome time.

Diſtinct ſtatement: that the alterations of rate are cauſed by wear in the 'ſcapement.

I therefore ſubmit it as a fundamental principle, that the principal cauſe of the errors found in the beſt adjuſted time-keepers, conſiſt in the wearing of the different parts of the 'ſcapement; that ſo long as by the act of wearing the relative proportions of its parts are preſerved, that errors of contrary kinds compenſate each other in the general action, the machine will go correctly; but ſo ſoon as thoſe ratios of the parts are altered by wearing, the watch will go either too faſt or too ſlow.

Short deſcription of the uſual detached ſcapement. 1. Scape wheel, 2. (face of) detent, 3. paſſing ſpring, 4 unlocking pallet, 5. impulſe pallet.

The ſcape wheel of what is termed the detached eſcapement, ſay of the late Mr. Arnold, or any other of them in uſe, (for their principles are the ſame, and differ only in modification,) is looked upon a jewel in the detent. The detent has a ſmall ſpring faſtened to it, which reaches conſiderably beyond the locking jewel. Some watch-makers have given the name of paſſing ſpring to it, becauſe, the little pallet in one vibration of the balance paſſes it without diſturbing the detent, and in the next vibration, the ſame pallet by laying hold of its point, lifts the detent out of the wheel, which immediately impels the balance by means of the great or impulſe pallet*.

Conſequences of wear. 1. If the wheel (point) ſhould wear, it will *ſooner* eſcape and ſtrike the pallet.

To ſimplify our deductions, let it be ſuppoſed that the point of the paſſing ſpring does not wear at all, and I think it will appear to every perſon acquainted with the 'ſcapement, that if the points of the teeth of the wheel ſhould wear, the watch muſt go faſter. For the wheel muſt then eſcape the locking jewel of the detent ſooner, and will neceſſarily impel the balance ſooner alſo; conſequently each impulſe given to

* Philoſ. Journal. Quarto II. 54.

5 the

the balance must be began quicker and quicker, as the points of the teeth of the 'scape wheel wear more and more. But on the contrary, let us now suppose that the points of the teeth do not wear, and the passing spring wears; in this case, as the balance in its vibration will have a greater space or portion of a circle to move, before it can arrive at the spring, and unlock the wheel, in order to receive the impulse, every such impulse will be made later and later, and the watch must necessarily go slower and slower as the point of that spring wears away more and more. There is another part in this 'scapement, namely, the striking face of the tooth, in which, if wear takes place, the tendency I presume will be to lose, in consequence of the wheel acquiring a greater drop, or having farther to go before it can overtake and strike the pallet. This part of the tooth I here supposed to wear, and which first comes into contact with the pallet, is not the point, but a little nearer the centre of the wheel; and as the wheel follows the pallet, it must be some portion of time longer before it can arrive to begin the impulse. In the above example I have supposed the detent and both pallets to be jewelled: if they were not, the errors would be greater.

If the passing spring wear, the unlocking, and consequently the impulse will be later.

If the face of the teeth should wear, the drop will be more and the impulse later.

Now we find that, according to the above reasoning, although there are two parts of the 'scapement in which, if wear takes place, the tendency will be to lose; leaving out of the question the oil becoming glutinous, the springs losing their elastic force, &c. and only one part, in which if wearing takes place the tendency will be to gain, yet, we find in most machines the tendency to gain is generally predominant. I will shew why I think the teeth of the wheel wear more than either of the other two parts: there is not only friction, but percussion also takes place in the three different parts of the 'scapement before-mentioned; the little roller or pallet that lifts the detent out of the wheel strikes against the point of the passing spring on coming into contact in order to unlock the wheel; in the next place the wheel strikes against the pallet when it begins the impulse, and lastly, the wheel again strikes against the jewel of the detent when it locks upon it: I will not attempt to determine which of the two former percussions is the greatest, or which will occasion the greatest retardation of rate, but will say, that the last is greater than either of the other two by much, and in general we find that the wheel is locked upon that

Thus there are two kinds of wear which tend to produce loss, and one to produce gain.

—but the gain predominates,

—and this because the stroke of the wheel in locking is much greater, that

those of the unlocking and the impulse.

that part of the face of the tooth neareſt the point; now as repeated ſtrokes will wear as well as friction, I take it that the wheel wears more from this cauſe (particularly where the execution has been favourable for it) than either of the other two parts, and therefore moſt time-keepers made upon this conſtruction have a greater tendency to gain than they have to loſe.

The advantage of clocks or machines with ſhort vibrations deduced from the ſmaller percuſſion.

If it ſhould be admitted that percuſſion will wear in any material degree, I ſubmit that it will lead to the diſcovering why clocks whoſe pendulums make the ſhorteſt vibrations go better than others, and alſo the cauſe of the great diſparity there is between the going of the common verge watch and that of table and long eight-day clocks; in both clocks and watches conſtructed with two pallets upon the recoiling principle, each pallet, with the accumulated force it has obtained by the vibration of the balance or pendulum, meets the wheel which is coming a contrary way when it comes into contact, and a percuſſion takes place; therefore the longer the vibration of the pendulum or balance, the greater muſt this percuſſion be, and conſequently the wear alſo: hence may be ſuppoſed one reaſon why ſpring or table clocks that make ſhorter vibrations go better than verge watches, and long clocks, which make ſhorter vibrations ſtill, go better than either.

Some objections conſidered.

I readily admit there is one, and perhaps many weighty objections to this reaſoning, viz. that ſome clocks upon the recoiling principle are found to go as well or even better than others on the principle of the dead beat, though the execution in each has been of equal excellence. For an anſwer to this objection, I refer the reader to Mr. Cumming's elaborate treatiſe upon clock making; who in ſome part of the work, aſſerts, the reaſon to be, that the plane of action of the pallet does not ſufficiently ſubtend the angle of vibration; by which means the power applied is adminiſtered too ſuddenly in the dead beat; to which I add that the percuſſion is alſo greater in this caſe in the dead beat than in the recoil, although the pallet does not meet the wheel in oppoſition, and the wearing of the teeth of the wheel may be increaſed by that means. There will be among many others one very ſerious objection to my ſuppoſition that the wearing of the wheel can produce error in a long pendulum clock, viz. the power of the pendulum being ſo much ſuperior to the motive force, it cannot be affected by it; to which I ſhall only obſerve that the reſults in practice are ſometimes very different from theoretical concluſions.

The

The mode of operation by which friction is faid to accelerate the rate of machines, is thus pointed out by the trade in general; viz. that whenever it takes place by the action of the wheel upon the pallet or pallets; (in watches or clocks conftructed with one or two pallets), or in the verge holes by the action of pivots therein, it impedes the motion of the balance or pendulum, fhortens its vibrations, and in confequence the machine goes fafter: how does this agree with Mr. Harrifon's opinion? which was, that large arcs are naturally performed in lefs time than fmall ones. Mr. Cumming, in the work of his before alluded to, has intimated, that nothing conclufive can be drawn from different arcs, becaufe they are effects and not caufes of error. Whether gain be occafioned by fhortened vibrations.

In the Repertory of Arts and Manufactures, No. 33, in the year 1796 or 7, there is a defcription of a Remontoire 'fcapement, which is completely detached from the train of wheels. The errors of this machine muft be determined to the 'fcapement alone, becaufe no errors in the going can poffibly arife in any other part of the watch. For if any additional power, however great, be applied to the train, the arcs of the balance will not be in the leaft affected by it: in the opinion of all or moft who underftood its principles, its going muft be more perfect than that of any machine before invented. An efcapement completely detached.

In the following remarks upon that 'fcapement, I fhall call, what is in the before-mentioned defcription of it termed a fnail pallet, a locking pallet. In the commonly termed detached 'fcapement, every tooth in the wheel is locked in fucceffion upon the jewel of the detent, and the effect arifing from wearing of the teeth does not very foon fhew itfelf, there being 12, 15 or more (at pleafure) in number, but in this remontoire 'fcapement before every impulfe there is only this individual locking pallet to fupply the place of a wheel, (if I may fo exprefs it) the wear therefore muft be 12 or 15 times as great, and its effect muft manifeft itfelf very foon. In this the wear on the locking is greater becaufe the action is on a fingle face.

In the years 98 and 9, I executed four or five of thefe 'fcapements, and tried the going of each. They all went in the fame manner, viz. for the firft twenty-four or thirty hours the machine would vary nothing; the day following it would gain one or two feconds, the next day feven or eight, and on the fourth or fifth day it would gain 30 or 40. I obferved, the more the machine gained, the larger were the arcs of the balance; Account of the conftruction and early performance of feveral of thefe.

lance; if the machine was set a-going without oil being applied to the locking pallet, it would not go many days before it would stop, and if set in motion the balance would perform only three or four vibrations, when it would come to rest again: in this state, if oil was applied to the locking pallet, it would set a-going more lively than ever, the balance making considerably larger arcs, and the machine gaining upon its former rate in a very surprizing manner.

Explanation of the irregularities;—from wear or change in the parts, and not from friction only.

Should it here be enquired, how were these effects produced? I think it would be answering in a vague and indefinite manner to say merely,—that it was occasioned by friction; for the mode by which it operated is certainly necessary to be pointed out: I suppose that by friction the point of the locking pallet (which was made of steel) wore shorter, by which means it locked shallower upon the jewel in the detent, therefore the balance, unlocking the Remontoire sooner, the impulse to the verge was given quicker as the point of the locking pallet became shorter by wear; also, the shallower the locking pallet locked upon the jewel of the detent, the less was the resistance to the balance in unlocking it, and the drop upon the impulse pallet became less, therefore the arcs of the balance became larger in proportion as the locking pallet wore shorter: I can never believe that the above effects, viz. acceleration of rate and large arcs were produced by friction abstractedly; for had it been so, they would have ceased the moment oil was applied to the locking pallet, instead of which the effects became more apparent.

Remedy. An intermediate wheel was introduced, to render the locking faces more numerous.

It was not till after various examinations of the 'scapement alluded to, and making two or three others of the same kind, that I formed the foregoing conclusions respecting the cause of the error that had puzzled me; when I had fixed upon the cause in my own mind, I determined upon a remedy, which was, by applying an intermediate wheel between the remontoire and the balance, instead of the locking pallet; I contrived to fix a short weak jewelled detent upon the remontoire axis, which moved round with it and locked upon the intermediate wheel, which had twenty-four teeth; as the locking pallet had before locked upon the detent, I placed also a detent upon the other side of the intermediate wheel, to lock it, and by a particular contrivance made the intermediate wheel to shift one tooth at every vibration of the balance; after which

alteration,

alteration, the former effects totally ceased; in order to make the intermediate wheel (which was loose) shift one tooth at every vibration, I made the detent which locked it strong, and inserted a long jewel into it, having an inclined plane that filled the space between each two teeth that always kept the wheel in a certain position, the consequence of applying the strong detent was, that the point of the passing spring (or in other words the unlocking part) wore very fast, and occasioned a very considerable retardation of rate, which continued without intermission until the watch stopped from the point of the passing spring wearing so much, that the balance could not unlock it. I must observe, that the arcs of the balance were in this case not perceptibly altered, and the retardation not nearly so great as the acceleration in the former. I afterwards, by another contrivance, which it would be difficult to explain, remedied the evil attendant upon a strong detent, and produced an escapement perfectly to my wishes, that had no propensity either to accelerate or retard its rate.

It succeeded.—Strong detent to shift the wheel —produced wear in the passing spring; —and retardation.

Remedy.

It is clear to me, that friction or wearing will, in some cases, make a machine accelerate, and in others retard its rate of going, therefore I presume if we could hit upon some method of preserving the ratios of the detached 'scapement permanent, by giving the different parts a durability, they have not hitherto possessed, we might produce machines of this simple construction that should retain an equal rate of going for long periods of time, and not look to chance for their success; if the actions of the 'scapement were unalterable (which I imagine might be made by jewelling) we should then have only the influence of the oil to oppose our success, which by becoming glutinous, will, I think occasion, if any change, a retardation of rate, yet if it be good, the effect, I suppose, will not be very great in a moderate length of time.

The simple detached 'scapement would perform well for long periods of time if preserved from wear.

Having been on the coast of Coromandel and Ceylon, and other parts of the East Indies, for a series of eight years, I had there an opportunity of trying an oil to watches, which artists here are unacquainted with; being in camp in the Mysore country, in the years 1791 and 2, for a period of about eighteen months, I could scarcely procure a drop of olive oil that was not rancid; I therefore substituted in room of it what is there called cocoa nut oil, which retains its fluidity to the last. I never knew an instance of its becoming glutinous, nor does

Cocoa nut oil recommended for time pieces.—

does it evaporate soon; I have found it in jewelled, verge, and horizontal watches, in that warm climate, after a period of three years, in a very fluid state, and in sufficient quantity. That oil may be easily procured here by breaking the shell of the cocoa nut, drying the nut in a very gentle manner, and afterwards expressing the oil from it, which is yielded in a great quantity; it assumes a concrete form in a very moderate degree of cold, but how far that may be a good objection against its use in this climate remains to be determined.

Mudge's first watch stopped for want of oil on the pallets.

I will conclude these observations, after a few remarks upon the stopping of Mr. Mudge's first watch, while under trial at the Royal Observatory; Mr. Mudge found, after a good deal of pains taken in investigating the cause, that it was for want of oil being put to the pallets (or locking part) of his 'scapement: I think this case so parallel to that of Mr. Haley's machines stopping from the same cause, that it tends in some measure to prove the truth of what I have advanced: Mr. Mudge had thought when he finished this machine, that it was altogether unnecessary to apply oil to this part, as he mentions in a letter to his excellency the Count de Bruhl, upon the subject of its stopping; and in the same letter he writes to the following effect, that when he had applied oil to the pallets, the watch went as well as before, or, as it did when first set a-going; that the arcs of vibration had fallen off but a few degrees, yet the watch went ten seconds a day faster than the last-mentioned rate, and from any thing that suggested itself to him, he could not see why it should not have gone rather slower than faster through length of time. I will venture to suppose the following to have been the cause of the machine going faster, and of the balance measuring smaller arcs in consequence.

Effect when oil w sapplied.

Explanations The teeth of the wheel wore shorter; escaped sooner, and (qu.) —

The points of the teeth of his 'scape wheel had become shorter * by constant wear, however small, and escaped the pallets (or rather hooks of the pallets, as they have been termed) sooner; therefore the opposite pallets which impelled the crank gave the impulse sooner to the balance, and in my mind, the watch may be supposed to have accelerated its rate from this cause; on the other hand, by the wearing of the teeth, the wheel did not wind up the remontoire springs quite so

* Refer to the figure, Philos. Journ. 4to series, Vol. II. p. 49.

high,

high, therefore there arose in some degree an impediment to the balance, which might occasion the arcs to be shortened, because, before the wheel was set at liberty, or unlocked from one pallet, in order to wind up the other remontoire spring, the balance (by means of the crank affixed to it) had alternately to wind up each remontoire spring a little higher, and in short, so much higher than the wheel had before wound it, as the momentum of the balance was equal to; therefore, as I said before, when the teeth of the wheel became shorter by wear, they would not wind up the remontoire springs quite so high; in which case the crank would engage the pallets on the opposite side of the remontoire axis a little sooner, by which means the balance would receive a check in its vibrations, and the arcs would certainly fall off. These, which I have mentioned, are the only reasons which have occurred to me to shew why the remontoire machine I made should have its arcs of vibration increased when the watch went faster, and, on the contrary, Mr. Mudge's should have its arcs shortened when it went the same way.*

—and the remontoire springs would be less wound up: whence the balance would have more to do alone.

As to Mr. Mudge's watches, *green* and *blue* retarding their rates while under trial at Mr. Dutton's, after having gone through their public trials at the Royal Observatory, it should be remembered they had not been cleaned, and that the oil being thicker, must have had a share in producing that effect, and perhaps a principal one; for as to friction in the impulse or unlocking part, there was none, it was totally annihilated, by the verge and remontoire axis moving on the same centre; if any wear could take place there, it must be by percussion and pressure only: likewise it may be considered, that the teeth of the wheel (or locking part) having been blunted by wear with former going, would have larger surfaces to resist the wear and its accelerating effect during this trial; besides, it will be readily believed by those acquainted with Mr. Mudge's scapement, that the wearing of his wheel and its pernicious effect, cannot be so great as in that of the detached 'scapement.

The retardation of Mudge's other time pieces ascribed to inspissation of the oil.

* It appears to me, that if the impelling pallet came to a stop before the instant of unlocking, no other than the last effect, in Mudge's machine, would take place; except that the resistance to unlocking might become less. Would the crank and levers wear and diminish the effect of the acting pallet? — N.

A Memoir

XIII.

A Memoir concerning the Fascinating Faculty which has been ascribed to the Rattle-Snake and other American Serpents. By Benjamin Smith Barton, M. D. *From the American Transactions,* Vol. IV.

(Continued from p. 285 of Vol. VII.)

Experiment of fascination by a snake.

THE facts which came under the notice of Mr. Vosmaër, at the Hague, are curious, and deserve to be mentioned. But they do not appear to me to be proofs of the existence of an infectious or mephitic vapour proceeding from the mouth of the rattle-snake. I am not at all surprised that the birds and mice that were put into the cage, along with this reptile, should exhibit the motions which were observed by the Dutch naturalist. When the little animals squatted down in a corner of the cage, they were, most probably, impelled by the instinct of fear, which is so powerful, and so extensive, in the vast family of animals. When they run towards the serpent, it may have been fear that actuated them.

Perhaps partially observed.

In conducting a series of experiments, it is ever a matter of importance, that the mind of the experimentalist should be free from the dominion of prejudice and system. Perhaps, facts are never related in all their unadulterated purity except by those, who, intent upon the discovery of truth, keep system at a distance, regardless of its claims. The strong democracy of facts should exert its wholesome sway. I cannot help thinking, that if Mr. Vosmaër had disbelived the fascinating faculty of serpents, the conclusions which he would have drawn from his experiments, just mentioned, would have been somewhat different. But of this I cannot be certain, and, therefore, I shall not avail myself of the supposition.

Other facts which contradict the notion.

Some experiments which have been made in this city, do not accord with those of Mr. Vosmaër. The birds, which were put into the cage that contained the rattle-snake, flew or ran from the reptile, as though they were sensible of the danger to which they were exposed. The snake made many attempts to catch the birds, but could seldom succeed. When a dead bird was thrown into the cage, the snake devoured it immediately. He soon caught and devoured a living mole, an animal much more sluggish than the bird. A few days since,

ſince, I had an opportunity of obſerving the following circumſtance. A ſmall bird, our ſnow-bird *, had been put into a cage containing a large rattle-ſnake. The little animal had been thus impriſoned for ſeveral hours, when I firſt ſaw it. It exhibited no ſigns of fear, but hopped about from the floor of the cage to its rooſt, and frequently flew and ſat upon the ſnake's back. Its chirp was no ways tremulous; but perfectly natural: it ate the ſeeds which were put into the cage, and by its whole actions, I think, moſt evidently demonſtrated, that its ſituation was not uneaſy.

No mephitic vapour was emitted by the ſnake in the experiment,

I do not relate this latter fact with any intention to diſprove the notion, that the rattle-ſnake poſſeſſes the faculty of charming. For the obſervation was made on the ſeventeenth of laſt month, which is ſomewhat earlier than the time when our ſnakes uſually come out of their dens. The ſnake, too, which was the ſubject of the experiment, appeared to be very languid, and had not eaten any thing for a conſiderable time. We ought not, therefore, to ſuppoſe him poſſeſſed of the faſcinating faculty at this period; ſince, I preſume, that this faculty, did it exiſt at all, is ſubſervient to the purpoſe of procuring the reptile its food. The fact is, perhaps, valuable in another point of view. It ſeems to ſhow, it does ſhow, that the mephitic vapour proceeding from the rattle-ſnake, allowing that ſuch a vapour really exiſted, was, in no reſpect, injurious to the bird.

nor in the wood at large.

If the mephitic vapour of the rattle-ſnake were productive of the effects attributed to it by Mr. de la Cépède, and other writers; and, eſpecially, if this vapour extended its influence to animals ſituated at a conſiderable diſtance from the reptile, the atmoſphere of the rattle-ſnake would often be a kind of Avernus, which many animals would avoid, and which would generally occaſion the ſickneſs or death of thoſe that were ſo unfortunate as to come within its ſphere. But how different is the caſe! The abodes of the rattle-ſnake are the favourite haunts of frogs, and many ſpecies of birds, which often paſs the ſeaſons of their amours and generation in clouds of mephitiſm; uninjured, and undeſtroyed. How often has the rattle-ſnake been known to continue, for days, at the bottom of a tree, or even a ſmall buſh, upon the branches of which

* The Emberiza hyemalis of Linnæus.

the

the thrush or cat-bird are rearing their young! This would be a suitable situation for the mephitic vapour to exert its noxious influence; but, in our woods, such influence has never been perceived.

Other instances in proof.

Birds of the eagle and the hawk kind have been seen to soar, for a considerable time, above the spot occupied by a rattle-snake, and at length to dart upon the reptile, and carry it to their young. Neither the parent-bird nor its young ones, have ever been known to receive any injury from the snake's vapour. Possibly it may be said, this vapour was dissipated, or greatly diluted, in passing through the air.

Whether other animals emit mephitic vapour.

A mephitic, or fetid, vapour emanates from the bodies of many animals, besides the rattle-snake; from the opossum *, and the pole-cat †, for instance. The vapour of these quadrupeds would be as likely to affect birds, &c. with asphyxy, as that of the rattle-snake. And possibly it does. There is, certainly, one thing in favour of the supposition. The opossum, in particular, is noted for his cunning in catching birds.

I shall conclude this part of my memoir by observing, that the odour of the rattle-snake is said to be agreeable to some persons.

Question, if the agitated animals may not have been already bitten.

Mr. de la Cépède's second mode of explanation is much more plausible. I have already observed ‡, that it was the system of Sir Hans Sloane, who affected to ground it upon experiments. It is adopted by the author of the well-written account of de la Cépède's *Natural History of Serpents*, in the *Monthly Review* §.

Mr. de la Cépède presumes that, "for the most part, when a bird, a squirrel, &c. has been seen precipitating itself from the top of a tree, into the jaws of a rattle-snake, it had been already bitten; and that its whole conduct, such as its crying, its agitation, its leaping from branch to branch, &c. are all effects induced by the violent operation of the poison, thrown into its body, by the reptile.

* Didelphis Opossum. † Viverra Putorius.

‡ See pages 30 & 34, *note*.

§ Appendix to the second volume of the Monthly Review enlarged. p. 511.

An

An attention to facts constrains me to reject this attempt towards a solution of the question, which I am considering. I shall arrange my chiefest objections under two heads.

Most probably not:

First. We are pretty well acquainted with the most prominent effects produced by the poison of the rattle-snake, in various species of animals. It must be admitted, that there is a considerable variety in these effects, and a great difference in the strength of these effects. In one animal, the poison produces an high degree of inflammatory action in the system; in another, the most striking primary effect is a somnolency, or drowsiness. In one animal, the poison does not produce any obvious effect upon the system for many minutes; in another the effects are almost instantaneous *. But in almost every instance in which the poison of the rattle-snake has been successfully thrown into the body of an animal, there ensue a set of symptoms, very different from the actions of birds and squirrels when under the supposed fascinating influence of the serpent-kind. It is not necessary to detail, in this place, these various symptoms, because I have already done it in a paper, which is printed in the third volume of the *Transactions* of our Society †, and because these symptoms cannot be unknown to the members of the Society. It will be sufficient to observe, that two of the most universal effects of the poison of the rattle-snake, I mean the extreme debility and the giddiness, which commonly almost immediately succeed the bite, will preclude the possibility of a squirrel's, or a bird's, dancing from branch to branch, flying about, and running to and from the serpent, for a considerable time, before it becomes a prey to its enemy. Besides, the farce of fascination is often kept up for a much longer term of time than any small animals are known to live after a successful bite by the rattle-snake. But, perhaps, it may be said, that the rattle-snake, like some of our wasps, knows how to inject into the animal, which he means to devour, any given quantity of his subtile poison. Here, the analogy will not apply: but I have not time to point out the various instances in which its failure is conspicuous.

—because the effects are more speedy and very different from what are related of fascination.

* A small dog that was bitten in the side by a large rattle-snake, reeled about, and expired, seemingly suffocated, in two minutes. This was in the month of August.

† No. xi. p. 110 and 11[illegible].

Kalm

The fyftem of fafcination implies that the animal may be relieved and efcape.

Kalm mentions a well-known fact, which will be admitted to have confiderable weight in deftroying the force of this part of Mr. de la Cépède's fyftem. "The fquirrel being upon the point of running into the fnake's mouth, the fpectators have not been able to let it come to that pitch, but killed the fnake, and as foon as it had got a mortal blow, the fquirrel or bird deftined for deftruction, flew away, and left off their moanful note, as if they had broke loofe from a net. Some fay, that if they only touched the fnake, fo as to draw off its attention from the fquirrel; it went off quickly, not ftopping till it had got to a great diftance." "Why," continues our author, "do the fquirrels or birds go away fo fuddenly and why no fooner? If they had been poifoned or bitten by the fnake before, fo as not to be able to get from the tree, and to be forced to approach the fnake always more and more, they could however not get new ftrength by the fnake being killed or diverted *."

(To be continued.)

SCIENTIFIC NEWS.

Very extenfive Table of Squares.

ABOUT three years ago, a large quantity of mathematical papers were brought to Oxford, which had belonged to Mr. Councer, an attorney at Bloxam, in the north of Oxfordfhire. They confifted chiefly of mere tranfcripts and collections from different publications: But there were two works among them, which muft be excepted from this defcription, and which prove Mr. C. to have been a man of fingular induftry.

The one was a table of fines and tangents for every fecond of the quadrant. It is probable, that this table was completed before the year 1760; but it would have been of no value, even if it had not been fuperfeded by Mr. Taylor's publication. For Mr. C. feems, in this inftance, to have been ignorant of the true method of calculating, and to have

* Travels into North-America, &c. vol. ii. p. 209 & 210. It will be eafy to difcover what part of Kalm's reafoning, in the above quotation I admit.

only

only interpolated ſome old tables, without allowing for the variation of the differences. The other work is really valuable, it is a table of ſquares and cubes to a much greater extent than that, which was publiſhed by the Board of Longitude in 1781, under the care of Dr. Hutton*. This table was calculated for the ſquares of all numbers from 1 to 128540, and for the cubes of all from 1 to 26560. But there is an interval in the ſquares from 28261 to 29061. It appears to have been originally left in the manuſcript, although it is impoſſible to conjecture the reaſon of the omiſſion. The grocer, who had purchaſed the papers, had begun to tear this table before it could be reſcued out of his hands; but as he had not got beyond the number 6000, this circumſtance is comparatively of little conſequence. In all other reſpects the work is complete. It is written in a clear hand, and (having been examined in ſeveral hundred places) appears to poſſeſs a degree of correctneſs, which may in general be depended upon.

Among the papers were propoſals for printing this table; they are dated 1758. There were, likewiſe, two ſheets of an introduction to them, printed in 1761. At this diſtance of time, it is impoſſible to aſcertain the reaſon, which prevented the publication; but it is not improbable that Mr. C. might have been ſtopped by the great expence of the undertaking. Had Dr. Hutton been in poſſeſſion of this manuſcript, it might have ſaved much of his valuable time, and if any man of ſcience ſhould be employed in continuing his work, it will be uſeful to him to know that Mr. Councer's has been preſerved.

P. S. Mr. Councer had, likewiſe, calculated all the powers from the 1ſt to the 13th (incluſive) of the ſeries of numbers from 1 to 20. And he has continued this, for the nine digits, as far as the 26th power. *S. R.*

*** I received the preceding notice from the reſpectable gentleman whoſe initials are ſubjoined; and am enabled to give ſuch farther information as may conduce to render the papers of public utility. *W. N.*

* Dr. Hutton's table is for the ſquares of all numbers from 1 to 25000, and the cubes of all from 1 to 10000.

Phoſphoric

On Phosphoric Rings; from a Correspondent: R. B.

Phosphoric rings.

I DO not know that any one has explained the phenomenon of those dense cloudy rings which are produced when phosphorated hydrogen arises through water, and explodes in the air; an effect similar to what is sometimes seen on the firing of cannon. Upon close inspection the fact appears to take place thus. The bubble of hydrogen, containing phosphorus in solution, rises and takes fire. Phosphoric acid is separated in the form of white smoke; through which dense mass the expansive action is directed upwards. A fluid opaque ring, denser than the atmosphere is thus formed, the inner surface of which is made to ascend by the rapid stroke, while the external part has received little or no impulse. The natural consequence is a quick rotation of the ring, from within outwards, which shews itself to the sight, and seems, in some manner or another, as if it kept the parts together.

How to measure the Contents of any Pipe by a very ready Method.

Contents of any pipe in pounds or gallons.

MY ingenious correspondent, Mr. Woolf, sometime ago gave me a ready method of measuring the contents of a pipe, as follows:

Square the diameter in inches, and the product will be the number of pounds of water in every yard length of the pipe. Or if the last figure be cut off or considered as a decimal, the remaining figures will give the ale gallons in the yard.

Dr. Winterbottom's Machine for clearing Roads from Mud.

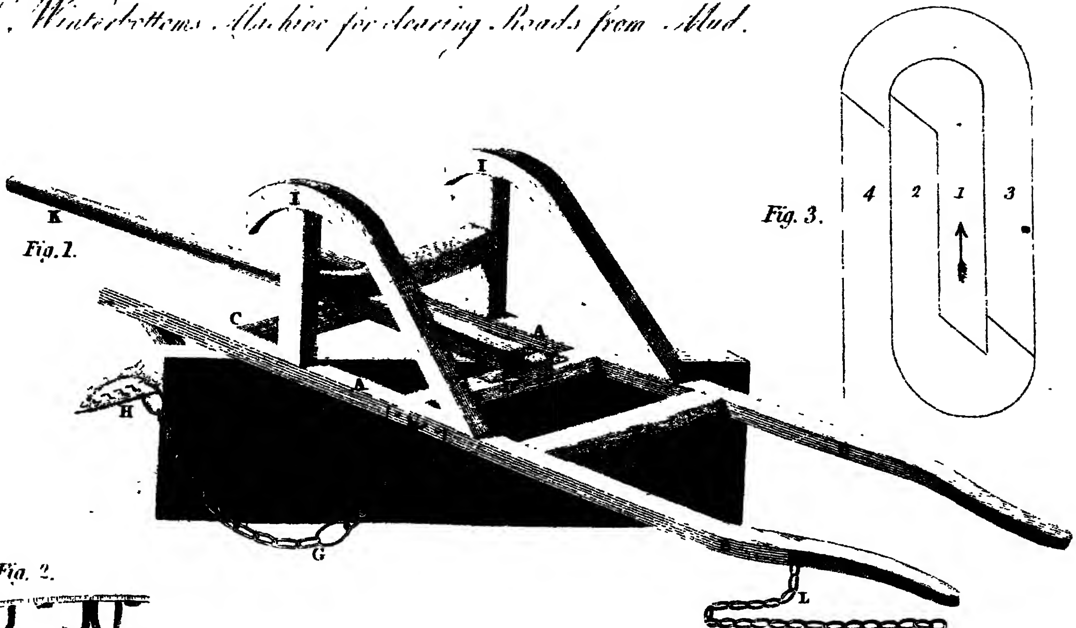


Fig. 1.

Fig. 3.

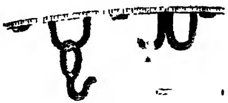


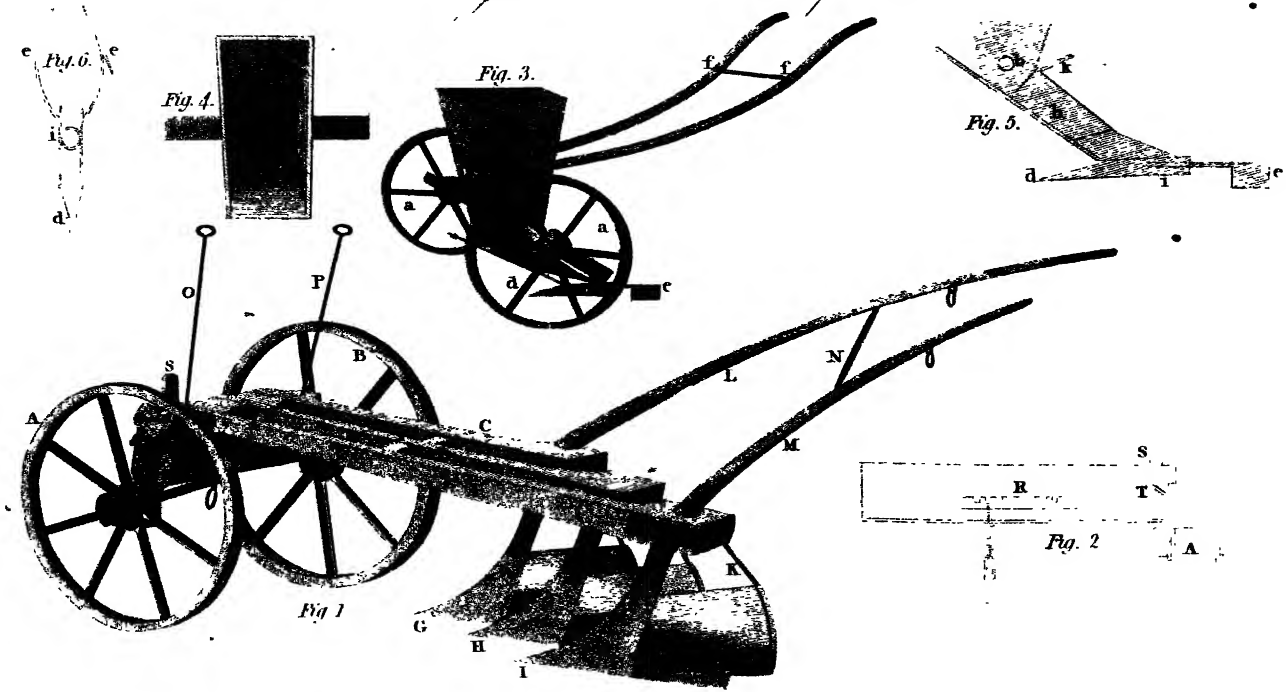
Fig. 2.

Scale of Feet.

7 14

Mutlow sc. Russel. C.

Mr. Robert Green's Hand Drill for Beans &c.



The Revd. E. Cartwright's three furrow Plough.

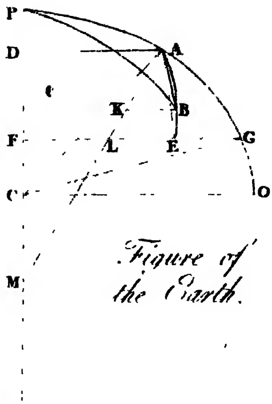


Fig. 1.

Figure of the Earth.

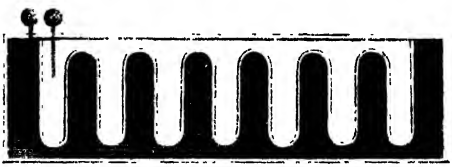


Fig. 2.

Galvanic Apparatus.

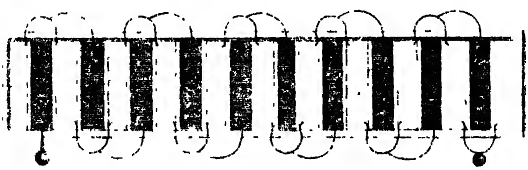


Fig. 3.

New Galvanic Trough. W. N.

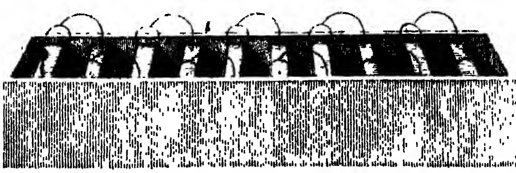


Fig. 4.

Perspective view of Fig. 3.

Hydraulic Machine.

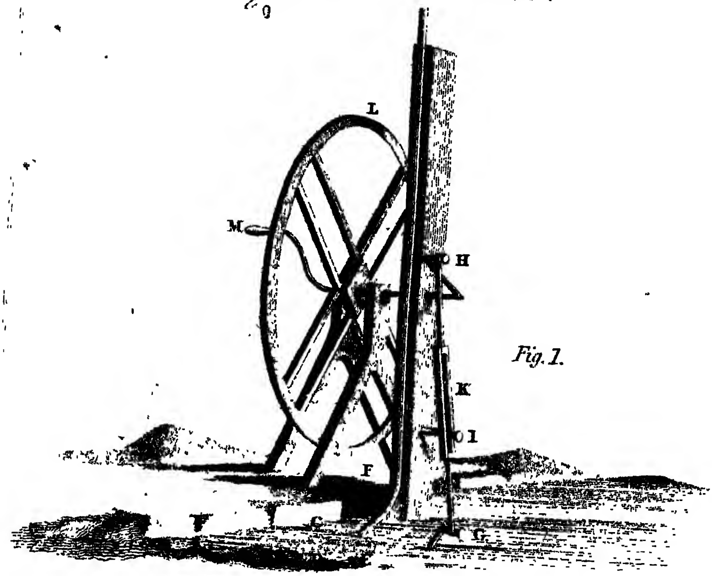


Fig. 1.

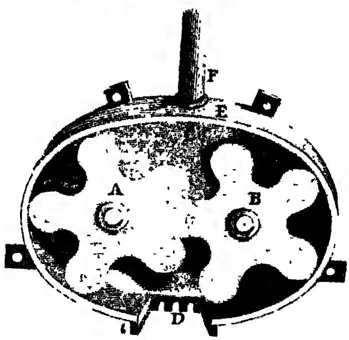


Fig. 2.

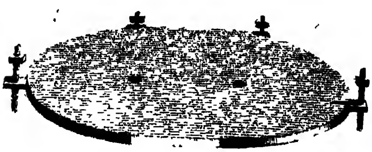


Fig. 3.

Drawn by Hunt

Engraved by Mutlow

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

JUNE, 1804.

ARTICLE I.

Letter from Mr. EZEKIEL WALKER, *on the Methods of observing the Longitude at Sea; with an Exhibition of the very great Accuracy of the mean Result from a Number of Chronometers.*

To Mr. NICHOLSON.

SIR,

THE two most approved methods of finding the longitude at sea, are those by lunar observations and by time-keepers. Although those methods are now brought to considerable perfection, yet they can only be looked upon as in a progressive state towards that point of precision, at which the astronomer and the artist have long been labouring to arrive. Longitude found by lunar observations and by time-pieces.

The lunar theory is still but imperfectly understood; but perhaps the time is at no great distance, when it shall be as well known as the theory of any other planet. Nor is it unphilosophical to suppose, that some property of matter, which is at present unknown, may hereafter be discovered, by means of which a time-keeper may be so constructed as to perform as well at sea as a regulator on shore. Both methods still improveable.

These are conjectures, though probable, that I shall not insist upon any further, but proceed to inquire into the merits of the two methods in their present state of perfection.

The lunar method will be for ever subject to those intervals wherein the moon cannot be seen.

The imperfection of the lunar method is, that in consequence of bad weather, or the moon's not being above the horizon, it sometimes happens that the necessary observations cannot be taken for many weeks. And these long intervals between one lunar observation and another, must for ever happen from the same causes; and those causes are such as no human art can remove.

Instances of the much greater frequency of observations by time-pieces.

La Perouse, in his voyage round the world, was at one time 31 days without taking a single lunar observation; but he had, within that time, 20 observations by his time-keeper. At another time during the same voyage, 24 days elapsed without a lunar observation; but he had 20 observations by his time-keeper. Lord Hugh Seymour, in his voyage to the West Indies in the year 1796, had been at sea near six weeks before the first lunar observation was obtained, in which period the longitude had been obtained 30 times by his time-keepers.

Time-keepers may stop; or may alter; or be neglected in winding up; or may suffer from accident.

What has just been mentioned sets time-keepers in a very favourable light, but it remains to inquire into their imperfections. 1. A time-keeper may stop. 2. It may alter in its rate of going. 3. It may not be wound up through forgetfulness. And, 4. It is liable to other accidents, even in the most careful hands. But these are defects which may be removed, and a result obtained, which will be more perfect than by any time-keeper that has yet been made.

Method of obtaining a more perfect result from time-keepers; proposed upwards of 20 years ago.

The method which I have to mention must be known to many gentlemen, as I had the honour to lay it before the commissioners of longitude in the year 1783. The method, however, seems either very little known, or not much attended to. La Perouse made no use of it in his voyage round the world; hence it may be supposed that it was unknown in France at that time. Nor is it once mentioned by Mr. Wales in his excellent treatise on the method of finding the longitude at sea by time-keepers; although he had "been pretty intimate with the subject for near forty years"*.

It consists in taking the mean result of a number of machines.

The method which I have recommended depends upon a particular use of time-keepers as they are now constructed. Although the best time-keeper be too imperfect a machine to be depended upon for determining the longitude in long

* Preface to Wales on the longitude, p. 5.

voyages, yet if five, fix, or more, be taken to fea in the fame fhip, the longitude computed by each feparately, and the mean of their refults taken, it will come exceedingly near the truth, even at the end of three or four months.

Another very great advantage refulting from this method is, that, fhould one of the time-keepers ftop, it may be fet going again by the reft; or fhould one of them be found to go very incorrectly, its rate may be rejected, and the longitude ftill determined by thofe which fhall be fuppofed to go well; in fhort, this method is almoft intirely free from all thofe accidents to which a fingle time-keeper is liable.

This method faves the time in cafe of neglect in winding up,

The two following examples, which are taken from the rates of chronometers that have been determined in fixed obfervatories, will fhow the perfection of this method more clearly. In the firft example, which contains a period of 30 days, there are only two inftances in which this method differs fo much as a fingle fecond from mean time. In the fecond example, containing a period of fix weeks, the greateft error on any one day, during that time, is only 5″.9, and the total error at the end of that period, is only 1″.9.

and affords a wonderful degree of precifion. Examples.

It may be faid that thefe are favourable fpecimens, but fuppofing that thefe chronometers had gone *ten times worfe* than they did, the greateft error in the firft example would have amounted to no more than 11″.3; and the greateft error in the fecond example would not have exceeded a fingle minute.

If the chronometers had performed ten times worfe, the error would have been lefs than one minute per month.

FIRST EXAMPLE.

Table of the going of three chronometers, and their mean result.

Day of the Month.	Mr. Arnold's Chronometers. Daily Rate of No. 36.	Daily Rate of No, 51.	Daily Rate of No. 59.	Mean Daily Rate of Three Chronometers.	Total Error of Three Chronometers.
	Feb. 1779	Feb. 1782	Feb. 1783		
1	+ 0″.79	− 0″.3	− 1″.5	− 0″.33	− 0″.33
2	− 0 .15	+ 1 .1	1 .5	0 .18	0 .51
3	+ 0 .15	− 0 .4	+ 0 .3	+ 0 .02	0 .49
4	0 .00	+ 0 .5	0 .7	0 .40	0 .09
5	− 1 .16	0 .0	0 .0	− 0 .39	0 .48
6	+ 1 .34	− 0 .7	− 1 .6	0 .32	0 .80
7	0 .81	1 .3	0 .4	0 .30	1 .10
8	− 0 .26	0 .0	+ 1 .4	+ 0 .38	0 .72
9	+ 0 .56	0 .5	0 .2	0 .09	0 .63
10	0 .07	0 .0	0 .6	0 .22	0 .41
11	− 0 .45	+ 0 .2	0 .0	− 0 .08	0 .49
12	0 .39	0 .3	− 1 .2	0 .43	0 .92
13	0 .25	0 .6	0 .4	0 .02	0 .94
14	0 .35	0 .9	+ 1 .4	+ 0 .65	0 .29
15	1 .48	− 0 .9	1 .2	− 0 .39	0 .68
16	+ 0 .09	0 .2	0 .2	+ 0 .03	0 .65
17	0 .30	0 .5	2 .2	0 .67	+ 0 .02
18	− 0 .38	0 .7	1 .4	0 .04	+ 0 .06
19	0 .77	0 .1	1 .6	0 .24	0 .30
20	0 .90	0 .9	− 0 .6	− 0 .80	− 0 .50
21	0 .00	0 .0	+ 0 .2	+ 0 .07	0 .43
22	1 .55	0 .0	1 .8	0 .08	0 .35
23	0 .48	+ 1 .0	0 .6	0 .37	+ 0 .02
24	0 .76	0 .7	1 .6	0 .51	0 .53
25	0 .29	0 .9	1 .2	0 .60	1 .13
26	0 .68	1 .0	− 1 .2	− 0 .29	0 .84
27	1 .74	1 .1	0 .8	0 .48	0 .36
28	0 .49	0 .1	+ 0 .5	+ 0 .04	0 .40
Mar. 1	1 .86	0 .4	0 .7	− 0 .25	0 .15
2	− 0 .80	− 0 .7	+ 1 .8	+ 0 .10	+ 0 .23

SECOND

SECOND EXAMPLE.

Table of three other chronometers.

	Mr. Mudge's Chronometers. Green Daily Rate	Blue Daily Rate.	Mr. Arnold's No. 86. Daily Rate.	Mean Daily Rate of Three.	Total Error of Three Chronometers.
1	− 1″.12	− 0″.27	− 0″.3	− 0″.56	− 0″.56
2	.61	+ .06	0 .1	.22	0 .78
3	.97	− .40	+ 0 .1	.42	1 .20
4	.61	.40	0 .3	.24	1 .44
5	.16	.16	0 .1	.07	1 .51
6	.11	.05	——	.05	1 .56
7	.64	.41	− 0 .8	.62	2 .18
8	.47	.57	0 .7	.58	2 .76
9	.35	.13	1 .1	.53	3 .29
10	.99	.45	0 .7	.71	4 .00
11	.05	.30	0 .1	.15	4 .15
12	.74	.37	+ 0 .1	.33	4 .48
13	.36	.12	0 .7	+ .07	4 .11
14	+ .04	+ .08	0 .2	.11	4 .30
15	+ .05	− .38	0 .5	.06	4 .24
16	− .26	.34	− 0 .6	− .40	4 .64
17	.00	.45	1 .2	.55	5 .19
18	+ .43	.18	0 .8	.18	5 .37
19	− .36	.49	0 .6	.48	5 .85
20	+ .07	.19	0 .0	.01	5 .89
21	+ .39	.23	0 .0	+ .05	5 .84
22	.14	.17	+ 0 .2	.16	5 .68
23	.59	.04	+ 0 .0	.18	5 .50
24	.49	.46	− 0 .2	− .06	5 .56
25	.65	.46	0 .3	.04	5 .60
26	.20	.71	+ 0 .1	.14	5 .74
27	.86	+ .03	0 .3	+ .41	5 .33
28	.53	− .41	1 .5	.54	4 .79
29	.74	.85	1 .3	.40	4 .39
30	+ .67	.07	0 .2	.27	4 .12
31	1 .50	+ .85	0 .6	.98	3 .14
1	.80	− .03	1 .8	.86	2 .28
2	.87	.23	1 .0	.55	1 .73
3	.63	+ .02	2 .6	1 .08	0 .65
4	1 .28	.16	0 .4	.61	0 .04
5	.43	− .51	2 .2	.71	+ 0 .67
6	.84	.42	0 .8	.41	1 .08
7	.61	.18	0 .6	.34	1 .42
8	.25	.68	0 .2	− .08	1 .34
9	.38	.54	− 0 .6	.25	1 .09
10	.41	.39	0 .2	.06	1 .03
11	− .83	− .27	+ 2 .2	+ .92	+ 1 .95

From

Hence the longitude at sea may be well determined,

From these examples we may see the nature of fortuitous events, how regularity rises out of irregularity; the error of one time-keeper correcting that of another in a wonderful manner; and as the number of time-keepers increases, the error will decrease, until it be almost annihilated. Consequently the method of finding the longitude at sea is no longer doubtful, as it may, by this method, be found to any degree of precision that may be useful in navigation.

and the expence is inconsiderable.

The only objection that can be advanced against it is the expence; but the sum of two or three hundred pounds bears no proportion to the value of a British squadron, nor even to the value of a single East India ship; and this sum would purchase as many time-keepers, at their present reduced prices, as would, I presume, be sufficient to secure any ship from that danger which might arise from the want of knowing the longitude.

EZEKIEL WALKER.

Lynn, April 19, 1804.

II.

On Galvanism. By C. WILKINSON, *Esq.*

May 6, 1804.

To Mr. NICHOLSON.

SIR,

Introductory letter.

BEING now engaged in some philosophical lectures at Bath, I have had frequent opportunities of conversing with Dr. Gibbes, a gentleman of considerable scientific information, relative to his opinions as to the apparent decomposition of water by the galvanic process. As many gentlemen of eminence in this department of philosophy are converts to his doctrine, I beg leave, through the medium of your valuable Journal, to state the outlines of Dr. Gibbes's theory to the public, with a few cursory observations which have occurred to me.

I am, Sir,

Your's with great respect,

C. WILKINSON.

No. 19, *Soho-Square.*

Soon

Soon after the publication of the valuable discovery of the decomposition of water by the galvanic process, by Messrs. Nicholson and Carlisle, Richter, an eminent German philosopher, observing that water becomes decomposed by wires placed at a considerable distance from each other, could not conceive that the same identical particle of water could be influenced by two wires at the same instant of time; and hence he conjectured that the Lavoisierean theory relative to water being a compound substance must be incorrect.

In the decomposition of water by galvanism the effects are produced at great distances asunder.

Dr. Gibbes supposes water to be an elementary principle, and that the two gases which are produced are compounds. Thus the oxygen gas which is disengaged from the zinc end of the battery, is taken to be a compound of water and positive electricity; the water constituting its ponderable part, while the hydrogen gas given out by the other wire connected with the copper end of the battery is concluded to have water also for its ponderable part in a state of combination with negative electricity.

Dr. Gibbes supposes the oxigen to be water and plus electricity, and the hidrogen to be water and minus electricity.

Dr. G. considers positive and negative electricity as two distinct principles, which when in a state of union constitute caloric. Thus in the explosion of a mixture of oxygen and hydrogen gases, the two electricities enter into union and form caloric, while the respective ponderable bases are precipitated in the state of water.

That plus and minus electricity compose caloric.

Upon this hypothesis, the circumstances of the evolution of flame and heat, together with the production of water, are capable of explanation.

Hence flame and heat,

Also upon this supposition some notion may be formed, why the gases are produced in the vicinities of each wire, and why oxygen gas must be evolved from the wire connected with the zinc end of the battery, and hydrogen gas from the wire united with the copper end.

and the galvanic phenomena.

This ingenious supposition, which does great credit to the well-known abilities of Dr. Gibbes, in the first point of view seems to receive additional support from its apparent simplicity. But upon more minute investigation, I am induced to think it will be found more complicated than the Lavoisierean theory, and by no means so general in its application.

Examination of the theory.

In the first place it should be proved that there are two such distinct principles as positive and negative electricity; and in support of this, the experiments of Eeles, Symner, Atwood, &c. are by no means conclusive.

The two electric principles not proved.

When

Nor the decomposition of ammonia explained.

When pure ammonia is exposed to the galvanic action, we have, as before, hydrogen from one wire, and nitrogen from the other. How can these productions be explained upon the principle of Dr. Gibbes?

This theory supposes oxidation to be the addition of water and subtraction of electricity.

This able physician further observes that oxydation is a combination of water with the metallic body, consonant to the opinions of Priestley, Watt, and others.

But the pile requires oxigen *from the atmosphere.*

When a galvanic pile is placed under a receiver situated over water, while the galvanic process goes on, by having a metallic circuit from the top to the bottom plate, a loss of air ensues, and which is found to be the pure part of atmospheric air: if the pile be placed in an exhausted receiver, the galvanic process is very trivial, and the plates very slightly oxydated. How upon the principles of Dr. Gibbes can we explain these phenomena? If oxydation be merely the combination of water with the metal, why should the pure part of atmospheric air be thus separated while the metals are in immediate contact with so much water?

Oxidation appears to require the disengagement of electricity.

Whether galvanism be the cause or the effect of the chemical change induced in the metallic substance, they appear to me to be contemporaneous results, similar to what appears in the immersion of iron in a solution of the sulphate of copper, and whether the iron be dissolved prior to the copious precipitation, no experiments I am aware of can decide. From various circumstances it appears that oxydation cannot take place unless the combined electricity of the metal be capable of being disengaged. This is rendered evident in a very happy experiment of Dr. Ash, who having been the associate of the much-lamented Humboldt* in his scientific pursuits, may now be deemed more acquainted with the minutiæ of galvanism than any other person. This gentleman has remarked that when a plate of zinc is immersed in a weak solution of sulphuric acid and water, a decomposition takes place, the oxygen base combines with the metal, while the hydrogen is disengaged in a gaseous state. When a plate of silver is immersed, no decomposition takes place; the very instant a contact is effected between the two metals, whether by portions out of the fluid or in the fluid, then the silver is immediately acted on and disengages hydrogen also, and itself becomes oxydated.

Dr. Ash's experiment.

* Humboldt died lately at Acapulco, of the yellow fever, while attempting to perfect his geological observations.

In

In the first instance the evolution of electricity appears to be the result of a chemical action of the sulphuric mixture on the zinc, while in the latter, no action is induced on the silver till some alteration is effected as to its electrical state; as we well know from other galvanic experiments, that of zinc and silver, when thus combined, the zinc evinces positive signs, and silver negative; it is hence evident that an abstraction of electricity from silver is requisite prior to its capability of being acted on by the sulphuric acid. Explanation.

If I might presume to submit to some of your chemical readers, an examination of such compound of silver as may thus be effected; a sulphate of silver is actually formed, it might lead to some important discoveries. I am every day more and more persuaded that light and electricity are the two most active agents we possess, and a more minute enquiry into their respective energies, I have no doubt, would enable us to explain many physical changes in the material world, with which we are at present perfectly unacquainted. Additional remarks.

The very intense light disengaged from charcoal by means of my extensive apparatus, has occasioned me to be frequently honoured with the attendance of Dr. Herschell; from some conversation I have had with him, I am in hopes he will be induced to institute some experiments relative to the evolution of light, so nearly approaching to solar light. If these should be conducted, I shall do myself the honour of stating them to the public through the medium of your valuable Journal. Intense light from galvanic combustion.

III.

On the Formation of Snow. By G. A.

To Mr. NICHOLSON.

SIR,

THE remarks contained in the enclosed pages, may not, perhaps, evince any originality of thought, nor more than would occur to the most superficial observer; yet the suggestions of some persons, frequently lead to the observation of valuable facts by others. Therefore, Sir, you will make whatever application of the paper you consider best.

If you think it worthy of insertion in your valuable Journal, it will be considered as an honorary obligation conferred on,

Your's, most respectfully,

23d April, 1804. G. A.

On the formation of snow;—

The frequent changes of the weather that have taken place during the last winter, having induced me to direct my attention to meteorology; I confess, that the manner in which philosophers account for some of the phenomena that occur, is not, to me, altogether satisfactory.

—supposed to be effected by electricity.

It is not surprizing, that electricity (with the immediate agency of which we are so little acquainted) should be resorted to, as the grand agent in all meteorological phenomena. Accordingly we find, that snow, and indeed every variety of weather we experience, is considered to be more or less effected by the electric fluid.

Snow is generally supposed to be the vapours of the atmosphere, disengaged by the electric fluid, and frozen.

Questions respecting the mode, &c.

But it appears to me, that before we receive so vague an explanation, the following questions might be asked:—

What are the vapours of the atmosphere composed of?

By what laws, and in what manner does the electric fluid act, either in the formation of snow, or as a component part of it?

Electricity supposed not essential to snow;

I shall now offer a few remarks to strengthen a supposition, that the electric fluid is not engaged in, or in the least essential to the production or existence of snow.

—but a change of wind.

By an attentive observation of all the circumstances that have attended the fall of snow, during the last winter, I have, in almost every instance, found that it is accompanied with, or rather preceded by a change of the wind; and that the wind, previous to the fall of snow, blew from some point between the *South*, and the *West*; and afterward from some point between the *East*, and the *North-West*.*

* If it is observed, that we sometimes have snow, without the wind changing to any of the points above-mentioned, or, even *without a visible change* to us; yet it does not militate against the following remarks; for it has been observed by aeronauts, that different strata of air blow from *opposite points* at the same time.—Therefore, notwithstanding a south wind may prevail at the surface of the earth, a superior stratum may blow from the North.

Such

Such being the facts, is it not probable, that *a change of the wind is the cause of snow?*

Now let us examine, whether such a cause will produce such an effect.

The winds that blow from any of the points between the *South* and the *West*, by coming from warm climates, and passing over, perhaps, a very large tract of water, where there is a powerful evaporation going on, must possess a very great degree of humidity, and are most commonly, of a temperature between 45° and 60° of Fahrenheit.

Warm winds meeting cold deposit their vapours in snow.

The winds which blow from any of the points between the *East* and the *North-West*, by coming mostly from such high latitudes, and passing over immense fields of ice, where evaporation is undoubtedly greatly impeded, cannot be supposed to contain much water in solution, but must bring with them very great degrees of cold.

Namely, north-easterly meeting south-westerly winds.

Now let us suppose that a north wind of any temperature between 32° and 0° (which it generally is, in superior strata of the atmosphere) meets a south-west wind, as before-mentioned, the consequence will be, that the intense cold which accompanies the former will convert the water with which the latter is impregnated into ice; and the instantaneous application of cold is probably the reason why snow is produced in what we call *flakes;* for before the vapour can concentrate itself into large particles, or drops, it is arrested by the intense cold.

In this view, the formation of snow appears to be a beautiful chemical phenomenon; for the warmer air, having a greater affinity for the colder air than it has for the water which is held in solution, the water is disengaged, crystallized by the cold, and precipitated in the form of snow.

Formation of snow.

It is generally observed, that it is unusually cold for half an hour or an hour before the fall of snow, and warmer afterwards. Might not this be accounted for, by considering that the adverse wind must meet with considerable resistance, in effecting either a *union* with, or a *passage through* a stratum of air surcharged with water, and consequently must be in a great degree reflected back again, not in the perpendicular, but as radii from a center, in an oblique direction, part of which must descend to the earth. And it will undoubtedly be warmer,

after

Formation of snow.

after the stratum of north wind has either forced a passage through or effected a union with the south-west wind.*

Though I have not, in the preceding observations, considered the electric fluid as at all essential to the production of snow, yet I do not deny the presence of it. That snow contains the electric fluid, cannot be doubted; but it does not follow, that the latter is *necessary* to the existence of the former. We know of no substance in nature, that is impervious to that subtile fluid; it seems to pervade *all bodies* with nearly the same facility as caloric. Therefore, though snow indicates electricity, it is probably no more than it has acquired in its passage through an electrified atmosphere.

If those who are much more competent to the task than myself, would direct their attention to this most interesting branch of natural philosophy, I am inclined to think, they would find the result of their enquiries highly gratifying. Meteorology cannot be considered, but as yet in a state of infancy; for the greater part, our knowledge of it is hypotheses, which we cannot support by experiment. Therefore, it is only by a close observation of facts, accompanied by just inferences drawn from them, that we can arrive at any degree of certainty on this complicated subject.

IV.

Medico Chemical Researches on the Virtues and Principles of Cantharides. By H. BEAUPOIL.†

Experiments and observations on cantharides.

THOUGH the animal kingdom presents us with only a small number of substances of use in medicine, it must nevertheless be admitted, that among those we possess, there are some of which the effect is so certain, so constant, and so definite, that if we were deprived of them; it would be difficult, and perhaps impossible, to find others to supply their place.

Cantharides are of this number; their mode of action is universally known, together with the advantages they afford in many disorders. It is not, therefore, to be wondered that the

* The water gives out heat in congelation. *Vide* Irwine, Black, Crawford, &c.

† *Annales de Chimie*, XLVIII. 29.

examination

examination of these insects should have engaged the attention of celebrated physicians, and that their analysis should have been frequently attempted by chymists. Experiments and observations on cantharides.

The principal aim of all those who have operated upon this material, has been to discover whether the blistering property which it so eminently possesses does generally appertain to all the parts of the animal, or whether it do not rather reside in some peculiar matter, which, independent of the parts which accompany it, is capable of acting alone, and producing the effects which are observable by the entire cantharides.

It is undoubtedly needless to repeat in this place, what has been said and done respecting this object; but it is essential to remark, that no one before Thouvenel pursued that course which could lead to the solution of the problem offered for consideration; and accordingly, we must consider the period in which that physician published his experiments on cantharides, as the earliest time in which philosophers could indulge the hope of ascertaining some positive information respecting the nature and properties of the immediate material of those insects.

But while we tender justice to the labours of Thouvenel, we must confess that he has not carried them to an extent answerable to his happy commencement. For he has neglected some of the most important questions, and among others, those which relate to the vesicatory, diuretic, and aphrodisiac properties of cantharides.

It was to supply in some measure the deficiency of that respectable philosopher upon the above three points, that Citizen Beaupoil has thought fit to undertake a new examination of cantharides. The paper in the annals consists of an extract from his memoir, by Citizen Deyeux.

The author divides his dissertation into four parts.

In the first he gives a rapid sketch of the specific properties of cantharides; the methods used for collecting them, and the preparations to which they are subjected previous to their introduction to the market as an article of commerce.

In the second he gives a slight history of the use and application of these insects from the time of Hippocrates to the present period.

In the third we find an accurate outline of the attempts made by chymists to analize the cantharides, as well as an account of his own particular experiments and their results.

The

Experiments and observations on cantharides.

The fourth contains every thing which relates to the physiological essays made with these animals; the effects produced by their exhibition, whether internally or externally; and lastly, observations relative to the opening of the bodies of several dogs, to which the author had given either the entire cantharides, or the different immediate materials, which he separated by means of his processes.

As the first and second parts contain nothing more than is to be met with in various authors, it is unnecessary to attend to them, and accordingly, the abridger has confined himself to the third part, in which the chemical facts are given.

Thouvenel, who was the first rational experimenter on cantharides, made use of water and alcohol to separate the soluble parts of these insects, and the results he obtained were,

1. A yellow reddish extractive matter, of a sharp bitter taste, resembling, as he says, that of ants, but less acid.

2. Another yellow matter, of a paler colour than the former, and nearly insipid.

3. A fatty matter, of a green colour and acrid taste, possessing the smell by which the entire cantharides are distinguished.

4. Lastly, A parenchymatous matter.

Citizen Beaupoil obtained similar products; but he not only ascertained their existence, but examined them separately, and in this it is that the difference between his labours, and those of Thouvenel principally consists.

He first observed that the aqueous solution of the peculiar extractive matter afforded by cantharides, does not fail to undergo a kind of alteration when exposed to the air; that the fluid becomes turbid, affords a yellowish precipitate, which acquires a peculiar odour; becomes covered with a viscid pellicle, emitting a fœtid ammoniacal smell; and that when it has arrived at this term, the fluid no longer exhibits any sensible change. He afterwards remarked that the solution here mentioned before it undergoes those changes which are produced by exposure to the air, strongly reddens the tincture of turnsole; that when mixed with rectified alcohol or ether, it becomes divided into two parts, nearly equal; the one possessing the form of a black adhesive precipitate, insoluble in alcohol, and the other that of a yellow brown matter, very soluble in that fluid.

(The Conclusion in our next.)

Description

V.

Description of a Galvanic Apparatus affording a large Surface for Oxidation, and convertible into one or more Plates at pleasure. By I. R. I.

To Mr. NICHOLSON.

SIR, *Edinburgh, May* 10, 1804.

I AM much flattered by the notice that you and Mr. Wilkinson have been pleased to take of my late communication, and I think I have done no small service to science by drawing forth from both of you, the very ingenious disquisitions towards the œconomy of galvanism, which appeared in your last Journal.

Your description of what may be called a Polychrest trough, seems to reach as near perfection, in every requisite property for giving the shock, or deflagrating metals, as could be wished: Yet there is still a further improvement which has suggested itself to my mind; but as it is probable the waste of the zinc would be more considerable, I mention it with diffidence. Improvement in the galvanic trough.

It appeared to me in some experiments I made with the *couronne de tasses*, that the metals were more corroded to produce the same effect, than happened either in the pile or the trough; but if Mr. W.'s idea is just (which is most likely to be, from his great experience in the science), that the effect is in proportion to the oxigenation of the metal, the following plan appears the best adapted for both œconomy and power. Corrosion greatest in the *couronne de tasses.*

In Fig. 3, Plate VI. A represents, by a side view or section, a plate of zinc of six inches by three; in its centre is a square piece, from which rises another piece of a smaller diameter, either square or round. These projecting bits are both sunk in a block of wood somewhat larger than the whole plate, but not so as to pass through it; and the plate must stand clear of the wood and every part of the cell, hanging entirely by the knob, and strongly cemented. A hore is made down through the thickness of the wood, in a line with one through the small knob of the zinc. Description.

The copper wire B, passed through that hole, is the medium of communication from cell to cell; and the same mode may be

be employed in forming them into one plate, as fuggefted, in your laft, when a deflagrating power is required.

The intermediate parts exprefs the wooden blocks, which muft be well covered with cement on every fide, and inferted into grooves, as the zinc and copper plates ufually are.

The engraving that referred to my laft communication, was inaccurately explained, as the dotted work was faid to exprefs the zinc, whereas the acid liquor was denoted by it.

I remain, Sir,

With much refpect,

Your obedient fervant,

I. R. I.

VI.

On the Difference between the Effects of Electricity and of Heat. By Cit. Berthollet*.

Difference between electricity and caloric.

I HAVE thought it important to determine the difference which exifts between the action of the electric fluid and that of caloric, and the caufe which renders their effects fimilar, more efpecially, as in the leffons of the Normal fchools this fimilitude of effect made me adopt the opinion of thofe who have confidered the electric fluid to be caloric itfelf; I confequently requefted permiffion of Citizen Charles to make ufe of his powerful apparatus in the experiments which appeared to me to be neceffary on this fubject. With that civility which thofe engaged in fimilar purfuits are always fure to experience from him, he undertook to perform them himfelf. I now give the refult, fuch as it was communicated to me by Guy Luffac, who affifted in the experiments.

Platina was not much heated by an electric fhock nearly capable of burning it.

A wire of platina was fubmitted to fhocks which were nearly ftrong enough to effect its combuftion; and to be fatisfied of this, a fhock was excited by which a great part of the wire was melted and difperfed; afterwards the fhocks employed were a little weaker, and immediately after each the wire was touched to judge of the temperature it had acquired: a heat was felt, which was diffipated in a few minutes, and which,

* From his Effai de Statique Chimique.

at

at the utmost, was estimated to resemble that of the boiling point of water. If electricity liquefied metals and brought them into combustion by the heat it excites, the platina wire must, after a shock which differed but little from that which would have produced its dispersion and its combustion, have approached the degree of temperature which occasions its liquefaction; now this degree, which is the most elevated that can be obtained, would, according to the valuation, more or less accurate, of Wedgwood, be 32277° of Fahrenheit.

When the shock is sufficiently strong to destroy the aggregation of the platina wire, it begins by detaching moleculæ from its surface, which exhale like smoke; if it is strong enough to produce combustion, the remains of the wire appear to be torn into filaments.

Electricity detaches the parts of platina, but does not fuse it.

A thermoscope, blackened with ink, and placed in the stream of a strong electric spark, only experienced a dilatation which was nearly equal to one degree of Reaumur's thermometer; and this slight effect might depend on the oxidation of the iron of the ink; placed beside the current, it did not shew any dilatation, although the air was necessarily affected by the electric action: it was the same when it was placed in contact with a metallic conductor which received a stream less powerful than in the preceding experiments.*

The electric current of sparks causes little heat.

A cylinder of glass filled with air, with an exciter at each of its extremities, to one of which was fixed a tube, communicating with another cylinder filled with water, produced an impulse at each shock which raised the water more than a decimeter above its level; but its effect was instantaneous.

Electric shock through air.

These experiments seem to me to prove that electricity does not act on substances, and on their combinations, by an elevation of temperature, but by a dilatation which separates the moleculæ of bodies. The slight heat observed in the platina wire is only the effect of the compression produced by the moleculæ which first experience the electric action, or which experience it in a greater degree; it must therefore be compared to that excited by percussion or compression.

Hence it is inferred, that electricity acts not by heating, but dilating.

If the dilatation was the effect of heat, that experienced by a gas in the experiment related above, would not have been

* A small thermometer in the luminous current between two balls of wood, is raised 32 degrees.—Nairne.

instantaneous, it would only have experienced a progressive diminution by cooling, as when its expansion is owing to heat.

When ammoniacal gas is decomposed by electricity, it remains cold.

In the experiment by which ammoniacal gas is decomposed, the gas indubitably receives the electric action, and nevertheless it is not heated; and as soon as the decomposition is finished, its volume remains unchanged, because the electric action which is employed in this experiment, is not sufficiently energetic to cause a perceptible dilatation. No sensible dilatation is produced in a gas by a shock which is not very strong, because the impulse not being gradual, like the expansion arising from caloric, and being excited instantaneously, the resistance of the liquid becomes too great, and cannot be overcome unless the dilatation has great energy.

Lead exploded in azote continues metallic, and is not fused.

An experiment of Dieman and his learned associates [illegible]ms this explanation: They caused a shock to pass [illegible] placed in a vessel filled with azote gas, which could [illegible] oxidate it; it was reduced into powder retaining al[illegible]etallic properties; If it had experienced a lique[illegible]tion similar to the action of heat, it would have cooled gradually, and would have congealed into one, or at least into several masses.

No heat is produced by the electric dilatation of metals: the ignition and heat are from oxidation or combustion.

When a metal is submitted to the electric action, the effects produced immediately by the electricity must be distinguished from those which are owing to its oxidation: The first are limited to the diminution or destruction of the effects of the force of cohesion, to removing and dispersing the moleculæ; (if by this a little heat is disengaged, it is only owing to the compression sustained by some of the parts); but those which are occasioned by the oxidation, produce a high degree of heat, and then the effects assume all the appearances of an ordinary combustion; hence it arises that the most oxidable metals are those which become red with the greatest facility, and which most shew the properties of a metal liquefied by heat.

Electricity favours oxidation by diminishing cohesion.

Electricity favours this oxidation, in as much as it diminishes the force of cohesion; it is thus that an alkali renders the action of sulphur on oxigen much more powerful, by destroying the force of cohesion opposed to it, and that a metal dissolved in an amalgam is oxided much more easily than when it is in a solid state. It is only by destroying the effects of the force of cohesion that heat itself produces the oxidation of metals, but the expansive action of electricity will have a great

advantage

advantage over that of caloric, because its action is confined to the solid which it encounters in its course, so that the gas itself will not experience a dilatation in opposition to the condensation which accompanies the combination: To this circumstance may be applied what is observed in the action of hidrogen gas, which is capable of completely reducing an oxide of iron placed in the focus of a burning glass, although water, whose two elements receive the heat equally, is decomposed by this metal.

Oxidation of metals by electricity and water.

It is probable that it is also to the expansive effect of an electric current established between two metals, having a stratum of water interposed, that the oxidation observed by Fabroni between these substances, placed in contact in water, is owing, and which, in this case, appears to be confined to the combination of the oxigen which is held in solution in this liquid *.

This explanation of the agency of electricity is general, but does not shew the difference between plus and minus.

All the chemical effects produced in substances submitted to the action of electricity, seem capable of being deduced from these considerations, and of being explained by the diminution of the force of cohesion, which is an obstacle to the combinations which their moleculæ tend to form; but the differences which may be offered by positive electricity and negative electricity remain to be determined: the chemical effects of the pile of Volta may be much more considerable than those of the common electricity, although the latter possesses a much greater tension, because its action being necessarily interrupted, the chemical effects which require time to be accomplished, can only begin to be effected, and may even be destroyed, by the sudden re-establishment of the first state of the body; while the permanence of the action of the electromotive apparatus, although weaker at each instant, may give rise to the chemical changes which it promotes, by diminishing the effects of the force of cohesion.

Galvanism may owe its greater energy to its constancy.

I do not myself consider the explanations I have now hazarded as more than conjectures, which observation may confirm or destroy.

* Journ. de Phys. Vendem. An. X.

VII.

Letter from a Correspondent I. R. I. *explaining some Facts in Galvanism, and on other Objects.*

To Mr. NICHOLSON.

SIR, *Edinburgh, May* 19, 1804.

Observations on galvanism.

I SENT you, a few days ago, some hints towards a farther improvement of the galvanic apparatus *, and I hope that the following observations will arrive in time to be added as a supplement to my last communication.

There seem to be two very marked laws in galvanism; the first is, that the phænomena are produced by moist oxidation, which seems a general one; but, by the second law, that they remain latent till one part of the conducting metal be made perfectly dry: this is at least strictly correct where two metals are employed.

From the tendency of galvanic effects to become latent, by the use of moist conductors, a pile cannot be formed unless one side of the oxidated metal be covered from moisture by cement, besides having a conductor of a different metallic substance. The trough is, therefore, best adapted to œconomize galvanism, as I have shewn; that nearly as large a surface can be exposed to the action of the fluid in it, as in the *couronne de tasses*, over which it has many advantages of steadiness, portability, &c.

From the necessity of dry conductors to alternate with moist to produce the more striking and perfect galvanic effects, it would appear that the former give the necessary celerity to their action; it might therefore be useful to enquire, whether making them of a considerable length, and inclosing them in some substance that would insure them from the moisture inseparable from the galvanic apparatus, might not considerably increase that celerity.

It is much to be wished that the end conductors were improved; jointed wires seem to lose much of the power, by not being in perfect contact; spiral wire seems better adapted to that use, and more capable of varying in its direction.

* See page 79.

It

It does not seem impossible to take a shock of its full force from piles or troughs of different powers, by bringing their conductors very near, but keeping them from actual contact by pieces of ivory or baked wood: it does not seem so clear that the full power of combustion could be preserved in the same manner.

Observations on galvanism.

I was lately rather surprized to find that galvanism had the effect of making silver remarkably brittle: this looks as if its action was somehow connected with the malleability of the perfect metals, and to shew that the conducting metals should be occasionally passed through the fire: the silver plates were nearly clean.

I will conclude with an observation on another subject. Having kept a considerable variety of Reeves's water-colour cakes in a damp closet, many of the cakes were in some degree affected by it so as to acquire mold; but the colour that Reeves sells under the name of Royal Smalt, has a most surprising power of attracting moisture, as it was reduced most completely to a soft mass; as almost all the vegetable, animal, and mineral substances used in painting were in the same box, we may pretty fairly conclude that an oxide of cobalt surpasses them all in its power of attracting moisture, and would make a most delicate hygrometer if suspended from one arm of a fine beam.

Attraction of moisture by oxide of cobalt.

I am, Sir,
With much respect,
Your obedient servant,
I. R. I.

VIII.

On the Presence of a new earthy Phosphate, found in the Bones of Animals, which does not exist in those of Men. By FOURCROY *and* VAUQUELIN. *Read before the National Institute.**

WE flatter ourselves the Institute will recollect our laborious essays on the analysis of urine, and stony concretions found in the body of man and animals; with the striking difference existing between them, and the cause to which the latter is owing.

Recapitulation of the analysis of urine, &c.

No phosphate in the urine of animals,

* Gehlen's New Journal of Chemistry, I. 555.

owing. We endeavoured to prove in thoſe eſſays that none of the ſalts called phoſphates, (which exiſt in abundance in the urine of man) are to be found in the urine of mamiferous animals, that their kidneys are not the concretory of theſe ſaline combinations; but that the hair which covers their ſkin, and the corneous appendages which defend their extremities, are the organs and repoſitories in which nature ſecretes and depoſits theſe ſalts in the body of animals.

Nor the morbid concretions called urinary calculi.

We have alſo ſhown that the morbid and preternatural concretions, called urinary calculi of man, contain beſides uric acid, alſo phoſphate of lime and phoſphate of magneſia; none of which are met with in thoſe of animals; and that on the contrary, the ſtony concretions found in the inteſtinal canal of animals, always contain phoſphates of different kinds, whereas the concretions met with in the inteſtines of man, do not contain a veſtige of them.

The bones alſo differ. Thoſe of animals contain phoſphate of magneſia; thoſe of man do not.

We ſhall now endeavour to prove that the bones of animals alſo differ in compoſition from thoſe of men. We have found that the former contain, beſides phoſphate and carbonate of lime, alſo phoſphate of magneſia; the latter of which has hitherto eſcaped the notice of chemiſts. This ſalt which we have detected in the bones of all the animals we hitherto examined, does not at all exiſt in the bones of man. We ſhall firſt ſtate the method we employed for detecting and ſeparating it, and then point out the relative proportions in which it exiſts in different animals.

Method of ſeparating magneſia from the bones of animals.

Take a quantity of bones of adult animals, burn them to whiteneſs in an open fire, and reduce them to a fine powder. Upon one part of this powder, after having been put into a convenient earthen or glaſs veſſel, affuſe an equal quantity by weight of concentrated ſulphuric acid; ſtir the mixture intimately together, and then ſuffer it to ſtand for five or ſix days. Having done this, dilute the maſs with at leaſt ten times its bulk of water, agitate it well and transfer it on a ſtrainer. When no more fluid paſſes, diffuſe the maſs again through five times its quantity of water, and ſtrain again as before, and repeat this proceſs till the water runs taſteleſs. The fluids thus obtained, are to be added together, and mingled with liquid ammonia, taking care the latter be in exceſs. The precipitate which is obtained conſiſts of phoſphate of lime, phoſphate of ammonia, and phoſphate of magneſia. To ſeparate

theſe

these salts, wash it in as little cold water as possible, and boil it in a solution of potash so long, till the odour of ammonia is no further perceptible. The potash in this process decomposes the phosphates of ammonia and magnesia, leaving the latter and phosphate of lime. To separate these two, let the whole be boiled in acetic acid, the magnesia will be dissolved, and the phosphate of lime remain untouched. Method of separating magnesia from the bones of animals.

To obtain the magnesia, let the solution of acetite of magnesia be carefully evaporated to dryness, re-dissolve the salt in water, and decompose the solution by carbonate of soda; the precipitate obtained is carbonate of magnesia. If it be pure carbonate of magnesia, it will be completely soluble in sulphuric acid; if it contains lime, the solution will be cloudy, and a precipitate will gradually be deposited.

Such is the method we employed for detecting and separating the magnesia contained in the bones of animals; it is perhaps tedious, but it is easy and certain.

The bones of the ox examined in this manner yielded something less than $\frac{1}{16}$ of its weight of sulphate of magnesia, which is equal to about $\frac{1}{74}$ of phosphate of magnesia, or in the burnt bone to $\frac{1}{24}$.

The bones of the horse and sheep afforded $\frac{1}{36}$ of phosphate of magnesia.

Those of fowls and fish yielded nearly the same quantity as those of the ox.

The results of a general analysis of the bones of the ox were:

Dry gelatine - - - -	51	0
Phosphate of lime - - -	37	7
Carb. of lime - - - -	10	0
Phosphate of magnesia - -	1	3
	100	0

The presence of phosphate of magnesia in the bones of animals, and its total absence in those of man, calls upon the physiologist to point out the source whence this salt is derived in the former, and why it is not met with in the latter. That it forms a constituent part of the food of both, we have proved elsewhere; that phosphate of lime enters into the composition of wheat, barley, oats, peas, &c. Why then is it not to be found in the bones of men? The nature of the human urine may perhaps assist in explaining this problem. We have proved that

that the urine of men contains phosphate of magnesia, and that the urine of animals is free from it. This salt is therefore ejected in man, by the kidneys; it therefore does not enter into the composition of bones; moreover the urinary calculi of man frequently contain phosphate of ammonia and magnesia, but no such salts are ever found in the stony concretions of the intestinal canal.

IX.

On the Nature of Oxigen, Hidrogen, Caloric, &c. as deduced from Galvanic Experiments. By A CORRESPONDENT.

On the existence of oxigen.

SINCE we are told that oxygen is one of the most essential substances in the production of the most striking phenomena of nature; that heat and light result from its change of combination, nay, that animal life is dependant on it, it becomes a matter of very great utility to investigate its nature, much more to prove whether it has any existence. It is well known that this substance, which has now so long and so generally been admitted, has never yet been exhibited except in combination, and that the evidence of its existence in combination has never been more than presumptive. It appears from some late experiments to be possible to substitute known for unknown principles, and to relieve science from those agents which are merely hypothetical.

Oxigen and hidrogen taken to be water with the power from each respective end of the galvanic wire.

The wires from the galvanic combination of metals produce different effects when placed in the same vessel of water. One produces inflammable air, the other vital air. When the circuit is made by the human body, a shock similar to an electrical one is perceived. It therefore appears that those powers which thus affect the human body, change water into inflammable and vital air. One wire always produces one air, and the other wire another air from water. Is it not therefore philosophical to refer the production of one of these airs to the power proceeding from one end of the pile and water; and the production of the other air to the power proceeding from the other end of the pile and water? In this experiment we are made sensible of no other principle, power, or substance than the above-mentioned. Why should we therefore have recourse to two hypothetical substances, oxygen and hydrogen, which have

have never in any experiment been made ſenſible to us? Is it not philoſophical to refer phenomena to cauſes which are objects of our ſenſes, rather than to account for them by agents which are merely hypothetical?

Under certain circumſtances water is converted into two airs, which airs have peculiar properties; in the galvanic experiment we are made acquainted with no other agent but thoſe powers which are elicited by the particular arrangement of metals, and theſe powers we are made ſenſible of; they produce different effects on various ſubſtances, and therefore I contend that theſe powers are different agents: for the ſame powers under the ſame circumſtances ſhould produce the ſame effects. The zinc ſide of the galvanic arrangement produces vital air, whilſt the copper ſide produces inflammable air.—Does it not appear from this experiment that there are other cauſes beſides caloric that give aeriform elaſticity to bodies? And do not the two powers of the pile here ſeem to be real principles? Each of them produces a real and decided effect on water. At all events we have not in this experiment any reaſon for aſſerting that water is a compound body, formed of two diſtinct and ſolid ſubſtances, oxygen and hydrogen. I do not mean at preſent to inquire whether negative electricity be a mere negation or not. We know that it is as much a cauſe of repulſion as what is called poſitive electricity, and that in experiments of a different kind from the one we are now conſidering. When a ſubſtance has in one inſtance been clearly proved to be formed of certain principles, it is conſiſtent with philoſophical accuracy to refer in all other inſtances to the ſame principles as the cauſes of the production of ſuch ſubſtance. Inflammable air therefore is water rendered aeriform by negative electricity or galvaniſm; and vital air is water rendered aeriform by poſitive electricity or galvaniſm. This is nearly the enunciation of the fact, and I contend that in this, and in all the reaſonings reſpecting water and fire, we have no occaſion for the two hypothetical principles, oxygen and hydrogen.

Developement of the facts and inferences.

In the above experiment with the pile of Volta, it appears that it is not caloric which cauſes the elaſtic aeriform ſtate of either the vital or the inflammable air, at leaſt we are not made ſenſible of it. If it were caloric that proceeded from the two wires of the pile, why ſhould each wire uniformly produce the ſame air, and one different from the other? There ſhould

The effect not cauſed by caloric.

be

be some reason why the solid base hydrogen attaches itself to the caloric of the one wire, and why the solid base oxygen is always so ready to enter into combination with the caloric of the other wire.

Compound nature of heat.

Scheele has asserted the compound nature of the matter of heat, and that all inflammable bodies contain a principle of inflammability, which principle, by combining with his igneous air, produces the phenomena of combustion. It does not appear that this opinion has been controverted by any decisive fact. We now know that some principle besides caloric is necessary in the production of one inflammable body, namely, of inflammable air; which principle, by combining with vital or igneous air, produces combustion. The re-produced water is common to both.

Generation of fire.

I think upon a further prosecution of this inquiry, it will appear, that fire is generated during combustion, that it is fire alone, (that is, the principle which causes the sensation of heat) that causes all the phenomena of combustion, and that it is, as Scheele observes, the water of his igneous air which forms the additional weight of bodies after they are burned. I know of no chemical fact that contradicts this explanation, in which no new substance that is not sensibly discovered is introduced.

A CORRESPONDENT.

X.

Experiments on the Yolk of Wool, followed by some Considerations on the Cleansing and Bleaching of Wool. By CIT. VAUQUELIN.*

Yolk of wool.

SEVERAL philosophers have thought that the yolk of wool was a fatty matter; others from its dissolving in water could not adopt the same opinion. Chemical analysis alone could decide this question, and this is what I proposed to myself in the work, the result of which I now offer.

Action of water upon wool.

1st. Water deprives wool of much of its colour, and the liquid acquires colour, odour and taste.

2d. The washings of the wool is milky like an emulsion of gum-resin, and passes through paper with difficulty.

* From the Ann. de Chimie, Fructidor, An. XI. No. 141.

3d. In

3d. In time it gives a depofition of fand, carbonate of lime, and feveral other foreign bodies; it lathers by agitation and heat like a folution of foap.

4th. The water with which wool has been wafhed, filtered and evaporated, yields a brown extract, thick like a fyrup, of an acrid, falt, and bitter tafte: in this ftate it ftill retains its peculiar odour. Aqueous folution and extract.

5th. Alcohol, applied to this extract, diffolves a part which communicates a reddifh-brown colour to it: if the alcohol be feparated from this fubftance by evaporation, it affumes the form of a tranfparent, thick, and vifcous honey. Extract expofed to alcohol.

The following are fome of the properties which it offered: To acids.

1ft. It diffolves eafily in water, and its folution is fpeedily coagulated by the acids, which feparate a fat fubftance from it, infoluble in water. The matter thus feparated by the acids, collects very flowly; its colour is yellowifh. The acids, as will be feen lower, retain a great quantity of it in folution, which gives them a reddifh brown colour. By evaporation, the greateft part of this fubftance, diffolved by the acids, is depofited in the form of a black bitumen, and falts are obtained with bafe of potafh and of lime. The greafy matter is fo adherent to thefe falts that they cannot be obtained in a ftate of purity and whitenefs, until after feveral calcinations and folutions. It yields falts with bafe of lime and potafh:

At the fame time that the acids precipitate this fat matter, they drive off a certain quantity of acetous acid, very diftinguifhable by its odour. Concentrated fulphuric acid blackens the infpiffated yolk, and difengages fome vapours of muriatic acid. Acetous acid, and muriatic acid.

2d. Lime-water renders the folution of the yolk turbid and milky, but it does not form a coagulum in it as in a folution of common foap. Lime-water.

3d. Cauftic alkalis or quick-lime do not demonftrate the prefence of ammonia. Cauftic alkalis and quick-lime.

4th. Nitrate of filver produces a yellow precipitate in it, which attaches itfelf to the fides of the veffel, like a *fat fubftance*. Great part of this precipitate is diffolved in nitric acid. Nitrate of filver.

The part of the yolk which is infoluble in alcohol has ftill a falt tafte, but lefs diftinct than the part which is foluble in this re-agent. After having been thus treated with alcohol, it does not entirely re-diffolve in water; there remains a glutinous matter, The infoluble part is ftill falt. Is not entirely foluble in water after having been treated with alcohol.

matter, of a grey colour, with which the acids produce a pretty brisk effervescence, which shows the presence of an alkaline carbonate. The portion which retains its solubility in water communicates a reddish colour and a saline taste to this fluid; its solution is not disturbed by the acids, as it was before having been treated by alcohol. Caustic alkalis do not disengage any ammonia; the muriate of barites forms a very abundant deposition in it, the greatest part of which is soluble in water: the nitrate of silver also occasions a precipitate in it, which dissolves partly in nitric acid. Alcohol precipitates this matter in the form of a mucilage, which is deposited quickly.

Contains an alkaline carbonate.

Action of reagents.

Nitrate of iron being mixed with the solution of this substance, formed a brown precipitate in it, and at the end of some days, the liquor furnished a pretty large quantity of nitrate of potash.

The yolk being decomposed by dilute sulphuric acid, and the liquor filtered, it blackened by evaporation, exhaled vapours of sulphuric acid, and became carbonaceous, as the concentration of the sulphuric acid took place. The residue being afterwards washed with water, and the solution suitably evaporated, yielded crystals of neutral sulphate of potash, but a good deal remained in the solution on account of the superabundant acid which brought it to the state of an acidulous salt: by a longer evaporation, this salt crystallizes in needles and plates of a pearly white.

Yields sulphate of lime by sulphuric acid;

During the course of these successive evaporations, another species of salt was offered, in the form of flattened needles, of a sattiny white, and without any sensible taste.

This salt examined with care, appeared to me to be only sulphate of lime; it however differed from it in some respects: for example, it melts much more readily by the flame of the blow-pipe into a globule, transparent while it is in fusion, and which becomes opaque by contracting: it is also much more soluble in water, and nevertheless does not contain the acid in excess; as I have satisfied myself. Its solution in water precipitates muriate of barites and oxalate of ammonia abundantly: one of these precipitates is sulphate of barites, and the other oxalate of lime. Neither lime-water nor ammonia disturb its solution. It appears therefore that this salt is a modification of sulphate of lime, which is probably produced by the proportion of its elements. It may also be possible that this salt still contains

tains ſome portions of fat matter, which, by decompoſing the ſulphate of lime, and forming a little ſulphuret would facilitate the fuſion. I regret my not having had a ſufficient quantity of this ſalt, to examine its properties more minutely.

The yolk diſſolved in water, filtered, and inſpiſſated, having been diſtilled with dilute ſulphuric acid, furniſhed a liquor in which I eaſily recognized acetic acid, by its odour, its taſte, and the properties of the ſalts which it formed with different baſes, particularly with lime and potaſh. Acetic acid;

Thus the yolk contains acetic acid, which without doubt is combined with part of the potaſh.

It [illegible] contains muriate of potaſh, for, with the ſolution of ſi[illegible] forms an abundant precipitate, which is not entirely ſoluble in nitric acid; and, by diſtillation with ſulphuric acid, it gives ſenſible indications of muriatic acid, which is mixed with the acetic acid. and muriatic acid.

The yolk evaporated to dryneſs, and ſtrongly heated in a ſilver crucible, ſwells, chars, and exhales fetid ammoniacal vapours; afterwards oily fumes ariſe which take fire, and when the greateſt part of the oil is diſſipated, it reddens, and enters into quiet fuſion. If, at this moment, it be poured on a marble, it yields a ſubſtance which contracts by cooling, of a greyiſh colour, and a very cauſtic alkaline taſte: if this ſubſtance be afterwards diſſolved in water, there only remains an infinitely ſmall quantity of carbonaceous matter, and, by evaporation, the liquor yields a true potaſh ſlightly carbonated.

It reſults from theſe experiments that the oil or greaſe, whoſe preſence in the yolk has been demonſtrated by means of the acids, is combined in it with potaſh, in the ſtate of a true animal ſoap; that, beſides, there is a portion of carbonate of potaſh in exceſs, ſince the acids produce a pretty briſk frothy efferveſcence in the concentrated ſolution of the yolk. In addition to the ſubſtances which I have juſt mentioned, the yolk contains a certain quantity of animal matter; for, by diſtillation, it gives very ſenſible traces of ammoniac, and an oil whoſe fetid odour reſembles thoſe furniſhed by animal matters. Recapitulation of the component parts;

The yolk is therefore formed, *1ſt*, Of a ſoap with a baſe of potaſh, which makes the greateſt part; *2d*, Of a ſmall quantity of carbonate of potaſh; *3d*, Of a perceptible quantity of acetate of potaſh; *4th*, Of lime, whoſe ſtate of combination

I am

I am unacquainted with; *5th*, Of an atom of muriate of potash; *6th*, finally, Of an animal matter to which I attribute the peculiar odour of the yolk.

which are not accidental.

I am of opinion that all these matters are essential to the nature of the yolk, and are not found in it by accident; for I have constantly found them in a great number of samples, as well of Spanish wool as of French.

I do not here speak of the other matters, insoluble in water, which are also met with in wool, such as the carbonate of lime, sand, and filth of every sort, these being evidently accidental.

Are they the products of cutaneous transpiration?

It remained now to enquire if all the matters in the yolk were the product of cutaneous transpiration, accumulated and thickened in the wool, or if they were taken up in the folds and other places in which the sheep lie. It is very certain that all the elements fit for the formation of the matters contained in the yolk, are found in the excrements of these animals, and in the vegetables which serve them for litter. Nevertheless, I could not believe that all of it was the effect of dung; on the contrary, I am of opinion that the humour of the transpiration is the principal source of it.

The analysis of the dung offers nothing certain in this respect, because the matters found in it may have been deposited there by the sheep themselves.

In what state are they emitted by the skin?

But admitting that the principles of the yolk arise from the cutaneous transpiration, which is very probable, are these matters emitted by the body of the animal in this state, and do they not experience some change while they remain in the wool? This is a question on which it is difficult to decide positively; we can only presume that changes are produced in it, as in all very complex substances deprived of motion, of which, in the present case, we neither know the cause nor the manner.

Washing the wool in running water is not enough to cleanse it.

The yolk, as we have seen above, being a true soap, soluble in water and alcohol, it would seem that nothing better can be done for scowering the wool than to wash them in running water. But I should observe that there is a small quantity of fat matter in the wool, which is not in combination with the alkali, and which, remaining attached to the wool, keeps it a little glutinous *(poisseux)*, notwithstanding the most careful washing.

But

But if the wool be put into buckets, and only as much water as will moisten it poured in, and if it be suffered to remain some time in this bath, pressing it often, it scours much better, and becomes much whiter afterwards, by washing in running water.

The scourers have a custom of macerating the wool in putrefied urine, and it is generally believed that it is the ammonia which is developed that effects the scouring; but I have some reason to think that this alkali is of no value. This effect is rather owing to the yolk itself, or to some other principle of the urine, to the urée, for example. The following are the grounds of my opinion in this respect; I put wool washed in running water into a mixture of sal ammonia and common potash; the mixture had a strong smell of ammonia, and nevertheless the wool was in no respect cleansed, because this alkali does not form, or at least with great difficulty, a saponaceous combination with the greasy matter of wool. From these observations, therefore, I believe putrid urine to be nearly useless in the scouring of wool, at least as far as respects its ammonia. Putrid urine does not promote the scouring.

Though the utility of putrified urine be in some degree doubtful, it is, on the contrary, very certain that fresh urine would be greatly injurious to the proposed object, for the soap contained in the yolk would incontestibly experience a decomposition by the acid of the urine, which would precipitate the grease on the wool. Fresh urine would precipitate the grease.

I suspect that the same effect would take place from washing the wool in water containing earthy salts, which are known to decompose alkaline soaps. For which reason it is always prudent to employ the purest water which can be procured for this purpose. As would water containing earthy salts.

This is not the case with soap-suds, which accomplish the scouring of wool perfectly, at the same time giving it more whiteness. If, therefore, after having washed the wool in running water till it loses no more, it be suffered to macerate for a few hours in only one twentieth of its weight of soap dissolved in a sufficient quantity of warm water, squeezing it often, it will be entirely purged of the small portion of grease which still adhered to it, and will then have a softness and degree of clearness which it could not have had without this operation. Soap-suds the best menstruum.

The

The yolk exercises an action on the uncombined grease.

The yolk itself, when a little concentrated, as I have already mentioned, has an efficacious action on the portion of grease which is not in a saponaceous state; for I have found that, in putting to the wool only the quantity of water necessary to cover it, it scours better, particularly with a little heat, than when it is washed in running water. But I also found that, when wool has remained too long in its own yolk, it swells, splits, and loses its strength: this effect also takes place with soap-suds which are too strong.

Danger of continuing its action too long; or of using strong soap-suds.

Since the solution of the yolk occasions this swelling and splitting of the wool, is it not possible that this accident may happen on the sheep's back, particularly in hot, moist seasons, or when they are shut up in folds in which the litter is not often enough renewed? Nor would it be impossible that the acrimony of the yolk should occasion an irritation in their skins, and, by that means, be the cause of some of the disorders to which this organ is liable in these animals, which must principally happen in hot and damp weather: fortunately, in these seasons, they are from time to time exposed to rains which wash them, and carry off at least a portion of this matter. On this subject I cordially agree with those who think that washing sheep in hot and dry weather, would be useful to their health and the quality of their wool.

The acrimony of the yolk probably hurtful to the living animal.

Washing recommended.

The loss experienced by scouring wool is very variable; the greatest I met with was 45 per cent. and the least 35; it it is true, those which I washed were very dry. This loss is not wholly owing to the yolk; the humidity, the earth, and the filth of every species, also contribute to it.

Loss of weight by scouring.

I have made some attempts to bleach scoured wool, but I confess that they have not been carried so far as they ought to have been. I have remarked, generally, that those which had been washed with soap-suds whitened better, by every method, than those which had not. Sulphureous acid dissolved in water whitened it pretty well, but it did not destroy the yellow colour which the wool, growing in the groin and under the fore-legs of the sheep, had contracted. In liquid sulphureous acid the wool acquires the property of crackling between the fingers like brimstoned silk, and, at the same time, contracts a very powerful fetid smell, which is not dissipated in a long time.

Bleaching of scoured wool.

I did

I did not try the vapour of burning sulphur, but all the world knows that it whitens wool well, and that the woollen manufacturers use it to give the finishing degree of whiteness to their goods. Of all the methods which I tried, I found none better for bleaching the wool, than exposing it, on the grass, to the dew and the sun, after being well scoured with weak soap-suds: the yellow spots of that from the groin, however, were not entirely destroyed; they had only diminished in intensity.

XI.

Copy of a Letter from Mr. CUTHBERTSON *to Dr.* PEARSON, *communicating an important and curious distinguishing Property between the Galvanic and Electric Fluids. Communicated by Dr.* PEARSON.

To Dr. PEARSON:

DEAR SIR,

I THINK it right to inform you, that yesterday evening I resumed the experiments with the galvanic batteries; the result was— Galvanic deflagrations.

1. Charcoal was deflagrated and ignited for about one inch in length.
2. Iron wire $\frac{1}{40}$ inch diameter, was melted into a ball $\frac{1}{16}$ inch diameter.
3. Platina wire $\frac{1}{100}$ inch diameter, was melted into a ball $\frac{1}{10}$ inch diameter.
4. Brass wire $\frac{1}{20}$ inch diameter, $\frac{1}{4}$ inch in length was ignited.
5. Ditto $\frac{1}{16}$ inch diameter, was red-hot at the extremity.
6. Iron wire $\frac{1}{150}$ inch diameter, was red-hot for 16 inches in length.
7. Ditto 12 inches, deflagrated and melted into a ball.
8. Ditto six inches in length, were deflagrated.
9. Ditto 8 inches in length, were ignited.

Two troughs, each trough containing 30 pair of plates six inches square, were used for the first seven experiments, and *one* of these troughs only for the two last experiments.

 The

Galvanic deflagrations.

The four last experiments prove, I think, that *double quantities of galvanic fluid only burn double* lengths of wire, and not the square, as electrical discharges do *.

I am, with the greatest respect,

Sir,

Your very humble servant,

JOHN CUTHBERTSON.

Poland-Street, Soho,
March 27, 1804.

XII.

Letter from a Correspondent, containing an Observation of the spontaneous Inflammation of Paper in Nitric Acid Gas.

To Mr. NICHOLSON.

DEAR SIR, *London, May* 22, 1804.

Paper inflamed in nitrous gas.

THAT several different inflammable bodies, while in a state of inflammation or of ignition, burn with an enlarged flame, and continue ignited when immersed in nitric acid gas, is, I suppose, commonly known; but that paper itself would take fire and flame most beautifully in this gas, and at not a very elevated temperature, has not, that I recollect, been already observed. By the following accidental circumstance this phenomenon was seen this morning in the public lecture-room, while reading on the subject of nitrous acid. In putting together the different parts of the Wolfe's apparatus, having ready only a bent tube much smaller than the lateral aperture of the globe condensing receiver, I filled up that aperture, partly with a piece of writing paper which projected into the receiver, and partly with almond paste. Soon after the acid had begun to distil, and while the apparatus was filled with

* It is not said whether the two troughs were used collaterally or longitudinally. Electrical jars may be considered as being always combined in the former mode. This subject seems to require comparison with the facts given in Mr. Wilkinson's letter in our Journal, Vol. VII. p. 207; but the communication came too late for me to offer any remarks upon it. W. N.

 reddish

reddish coloured nitrous acid gas, I was surprized by the bursting forth of flame from the paper, which was consumed by it in less than a minute, without cracking, as I expected, the receiver. I think it unnecessary to make any comments, or give the *rationale* of this fact.

Always your's faithfully,

AMICUS.

XIII.

Description of a Jib on a new Construction; by Mr. J. Bramah, *Engineer. Communicated by the Inventor.*

JIBS of the usual construction turn on two solid gudgeons. The rope by which the goods are raised, passes over the upper gudgeon, and is confined between two small vertical rollers, in order that it may constantly lead fair with the pulley or sheave at the extremity of the jib. According to this construction, whenever the crane turns round its axis, the rope is bended so as to form an angle more or less acute, which causes a great increase of friction, and produces a continual effort to bring the arm of the jib into a parallel position to the inner part of the rope. These inconveniences may appear to be trifling on paper, but in actual practice they are of no small importance, for they necessarily imply a much greater exertion of power in raising goods, and the application of a constant force to keep the jib in the position that may be requisite; while the partial stress which is exerted on only a few strands of the rope, when bended into an acute angle, infallibly destroys it in a very short time. Description of a jib on a new construction.

The simple construction exhibited in Plate V. obviates all these defects, and at the same time possesses the very desirable property of permitting the jib of what is termed a campshut or landing crane, wholly to revolve round its axis, and to land goods at any point of the circle described by the arm of the jib.

It consists in perforating the axis or pillar of the crane, and in conducting the rope through this perforation by means of an additional pully fixed on the top of the arm of the jib.

 The

Defcription of a jib on a new conftruction.

The nature of the contrivance cannot fail to be fufficiently underftood by an infpection of the figures; the one of which reprefents a jib attached to the wall of a warehoufe, the other a campfhot or landing crane fixed on the edge of the wharf. Each of thefe jibs turns on a perforated axis or pillar. The rope proceeds from the goods which are hoifted, through a pully fixed as ufual at the extremity of the jib; it then paffes over another pully fixed at the oppofite extremity of the jib, and is, by this pully conducted through the perforated axis or pillar to a third pulley; whence it is immediately directed to the crane by which the weight is elevated.

It is almoft unneceffary to ftate that the lower axis is ufually fixed in an oil box, and that friction rollers are applied to the axis wherever the circumftances may render it neceffary.

The importance of this improvement, in an article of fuch extenfive ufe, muft be evident even to thofe who are the leaft acquainted with the fubject. Mechanics who are aware that fimplicity of conftruction and certainty of effect are among the moft valuable characters to be fought in engines, will moft probably obferve this crane with pleafure; and the advantages to the community at large muft be meafured by the convenience and faving of labour it is calculated to afford.

XIV.

A Memoir concerning the Fafcinating Faculty which has been afcribed to the Rattle-Snake, and other American Serpents. By Benjamin Smith Barton, M. D. *From the American Tranfactions,* Vol. IV.

(Concluded from Page 62.)

Other fnakes (particularly the black-fnake which is not poifonous) are faid to charm.

SECONDLY. It is a fact well known in this country, that the rattle-fnake is not the only kind of ferpent that is faid to be endued with the faculty of fafcinating birds, fquirrels, and other animals. As far as my inquiries have extended, it does not appear to me that, in general, the rattle-fnake is thought to have fo large a portion of this faculty as fome other fpecies of ferpents. Of this, at leaft, I am certain, that perfons refiding in our country-fituations tell us many wonderful tales of the bewitching eyes of the black-fnake, the coluber conftrictor of

of Linnæus, as they do of the boiquira, or rattle-snake. Now let it be supposed, for a minute, that the poison of this latter serpent, when thrown into the body of a bird, a squirrel, &c. is capable of producing, in these animals, those piteous cries, those singular movements, those tremulous fears, which are mentioned by Kalm, by de la Cépède, and by other writers,—in what manner are we to account for the similar cries, movements, and fears, in those birds which are frequently seen under the fascinating influence of the black-snake? For we Americans all know, that the bite of the black-snake is perfectly innoxious. This, indeed, is also the case with the greater number of the species of serpents that have, hitherto, been discovered in the extensive country of the United States. And yet almost every species of serpents is supposed to be endued with the power of fascinating such animals as it occasionally devours.

These facts, and this mode of reasoning, certainly involve, in some difficulty, Mr. de la Cépède, and those writers who espouse his opinion, which I have examined, under the first head of my objections. An attempt is made to account for the imaginary fascinating faculty of the serpent from the powerful influence of a subtile poison. But, upon inquiry, it is found, that the power of bewitching different animals is not an exclusive gift of those serpents which nature has provided with envenomed fangs: it is a gift which as extensively belongs to that more numerous tribe of our serpents, whose bite is innocent, and whose creeping motion is their only poison *. These

and consequently the supposition of poisonous influence is unfounded.

* If there is any impropriety in this mode of expression, the impropriety has its source in my feelings, with respect to the serpents. Perhaps, no man experiences the force and the miseries of this prejudice in a greater degree than I do. It is the only prejudice which, I think, I have not strength to subdue. As the natural history of the Serpents is a very curious and interesting part of the science of zoology; as the United-States afford an ample opportunity for the further improvement of the history of these animals, and as I have, for a long time, been anxious to devote a portion of my leisure time to an investigation of their physiology, in particular, I cannot but exceedingly regret my weakness and timidity, in this respect. I had meditated a series of experiments upon the respiration, the digestion, and the generation of the serpents of Pennsylvania. But, I want the fortitude which it is necessary to possess in entering on the

These objections will, I am persuaded, be sufficient to convince every unprejudiced reader, that the system of explanation offered by Mr. de la Cépède is unfounded in facts; and, consequently, that the problem still remains to be solved, in another way.

Professor Blumenbach admits the fact; but ascribes it to a moral cause.

Among the number of ingenious men who have amused themselves with speculations on the subject of this memoir, and who, rejecting the commonly received notion of the existence of a fascinating power in the rattle-snake, have attempted to explain the phenomenon upon other principles, it is with pleasure I recognize the respectable Professor Blumenbach, of Gottingen. This gentleman, in a late publication, speaking of the rattle-snake, makes a few remarks on the fascinating faculty which has been ascribed to this reptile. These remarks I shall translate at length.

Toads, hawks, &c. are said to have the power of fascinating by the eye.

"That squirrels, small birds, &c." says he, "voluntarily fall from trees into the jaws of the rattle-snake, lying under them, is certainly founded in facts: nor is this much to be wondered at, as similar phenomena have been observed in other species of serpents, and even in toads, hawks, and in cats, all of which, to appearance, can under particular circumstances, entice other small animals, by mere stedfast looks. Here the rattles of this snake (the rattle-snake) are of peculiar service; for their hissing noise causes the squirrels, whether impelled by a kind of curiosity, misunderstanding, or dreadful fear, to follow it, as it would seem, of their own accord. At least," continues Mr. Blumenbach, "I know from well-informed eye witnesses, that it is one of the common practices among the younger savages to hide themselves in the woods, and by counterfeiting the hissing of the rattle-snake to allure and catch the squirrels."*.

the task. Instead of slowly and cautiously dissecting and examining their structure and their functions, with that attention which the subject merits, I am more disposed, at present, to obey the injunction of the Mantuan poet, in the following beautiful lines:

———Cape saxa manu: cape robora, pastor,
Tollentemque minas et sibila colla tumentem
Dejice: jamque fuga tumidum caput abdidit alte,
Cum medii nexus, extremæque agmina caudæ
Solvuntur, tardosque trahit sinus ultimus orbes.

GEORG. Lib. iii. 420—424.

* Handbuch der Naturgeschichte, P. 253. Gœttingen: 1791.

I do

I do not intend to take up much time in examining the foregoing explanation. I shall offer my objections to it, in as concise a manner as I can.

Examination of the facts stated by Blumenbach, which are contested.

First. The faculty of fascinating is by no means peculiar to the rattle-snake, but is attributed as extensively to the black-snake, and other serpents, which are not furnished with the crepitaculum, or set of bells *, by which this serpent is supposed to be enabled to ring for its prey, when it wants it.

Secondly. Some persons, who have seen the rattle-snake in the supposed act of charming, assure me that the reptile did not shake its rattles, but kept them still. It is true, that Mr. Vosmäer's rattle-snake, already mentioned, continually shook its rattles.

Facts and observations respecting the power of fascination ascribed to snakes.

Thirdly. With regard to the practice of the young savages, spoken of by Mr. Blumenbach, I know nothing. I have inquired of Indians, and of persons who have resided for a considerable time, among the Indians, and they appear to be as ignorant of the circumstance as I am myself. I am inclined to think that Mr. Blumenbach has been imposed upon: or, perhaps, the following circumstance may have given rise to the story. The young Indians put arrows, across, in their mouths, and by the quivering motion of their lips upon the arrows, imitate the noise of young birds, thus bringing the old ones so near to them, that they can be readily shot at. In like manner, the Lanius Excubitor, or great shrike, hiding itself in a thicket, and imitating the cry of a young bird, often succeeds in seizing the old ones, which have been solicited, by the counterfeited noise, to the assistance of their young.

Ever since I have been accustomed to contemplate the objects of nature with a degree of minute attention, I have considered the whole story of the enchanting faculty of the rattle-snake, and of other serpents, as destitute of a solid foundation. I have attentively listened to many stories, which have been related to me as proofs of the doctrine, by men whose veracity I could not suspect. But there is a stubborn incredulity often attached to certain minds. In me it was strong. The mere force of argument never compelled me to believe. I always suspected, that there was some deficiency in the extent of observation, and the result of not a little attention to the subject has taught me, that there is but one

* Serpent à sonnette is the French name for the rattle-snake.

wonder

Facts and observations respecting the power of fascination ascribed to snakes.

wonder in the business;——the wonder that the story should ever have been believed by a man of understanding, and of observation.

In conducting my inquiries into this curious subject, I thought it would be proper, and even necessary, previously to my forming a decided opinion, to ascertain the two following points, viz. first: what species of birds are most frequently observed to be enchanted by the serpents? and, secondly, at what season of the year has any particular species been most commonly seen under this wonderful influence? I was induced to believe that the solution of these two questions would serve as a clue to the investigation of what has been long considered as one of the most mysterious operations in nature. I am persuaded that I have not been mistaken. Possibly, the credulous may not think as I do.

It is a curious circumstance in the history of birds, that almost every species, in the same country at least, has an almost uniform and determinate method of building its nest, whether we consider the form of the nest, the materials of which it is constructed, or the place in which it is fixed*. Some observations on this subject are necessarily connected with the point under investigation, in this memoir;—indeed, they are involved in the question concerning the species of birds which have most generally been observed to be enchanted by the rattle-snake, &c.

Some birds build their nests on the summits of the loftiest trees; others suspend them, in a pendulous manner, at the extremity of a branch, or even on a leaf†, whilst others build them

* I do not mean, by this observation, to assert, that birds are necessarily impelled to construct their nests of the same materials, or to place them in the same situations; yet such is the language of some writers on natural history, and on morals, who talk of the "determinate instinct" of animals, and who think it impossible that "animals of the same species should any where differ." "The grouse in America, we are told, perch upon trees; the hare burrows in the ground; and we have, in these instances, sufficient reason to deny that the species of either is the same with those of a like denomination, with which we are acquainted, in Europe." These are the words of the celebrated author. See Dr. A. Ferguson's Principles of Moral and Political Science, vol. i. p. 59 & 60. quarto edition.

† See a very interesting account of the Motacilla sutoria, or Taylor-

them on the lower branches, among bushes, and in the hollows of decayed, and other trees. Many species, again, are content with the ground, laying their eggs, and hatching them, in the cavity of a stone, an excavation from the earth, among the grass of fields and meadows, or in fields of wheat, rye, and other grains. Thus, to confine myself to our own country, the eagle, the vulture, the hawk, and other birds of this extensive family, make choice of the loftiest oaks, and other trees of our forests; the baltimore-oriole *, commonly called, in Pennsylvania, the hanging bird, suspends a beautiful nest to the extremity of a branch of the Liriodendron †, or some other tree; the migrating thrush ‡, called robin, is content with the lower branches; the red thrush §, the cat-bird ||, the red-winged oriole, called the swamp-black-bird ¶, and many others build in the low bushes; the wood-peckers **, the blue motacilla (blue-bird) ††, the torchepot ‡‡, and others, build in the hollows of trees, the chattering plover §§, and the whippoor-will ||||, take advantage of a hollow place in the ground, or in a stone, which the great lark ¶¶, the marsh-wren ***, &c. place their nests in the grass; and, lastly, the partridge ††† builds in the corn-fields.

Facts and observations respecting the power of fascination ascribed to snakes.

Of all these birds, and of a great many others, those which build their nests upon the ground, on the lower branches of trees, and on low bushes (especially on the sides of rivers, creeks, and other waters, that are frequented by different kinds of serpents), have most frequently been observed to be under the enchanting faculty of the rattle-snake, &c. Indeed, the bewitching spirit of these serpents seems to be almost entirely limited to these kinds of birds. Hence, we so frequently hear tales of the fascination of our cat-bird, which builds its nest in the low bushes, on the sides of creeks, and other waters, the most usual haunts of the black-snake, and

Taylor-bird, by my learned friend Mr. Pennant, in his Indian Zoology, pages 44, 45 & 46.

* Oriolus Baltimore.
† Liriodendron tulipifera.
‡ Turdus migratorius.
§ Turdus rufus.
|| Muscicapa carolinensis.
¶ Oriolus phœniceus.
** Pici.
†† Motacilla Sialis.
‡‡ Sitta.
§§ Charadrius vociferus.
|||| Caprimulgus.
¶¶ Alauda magna.
*** Motacilla Troglodytes?
††† Tetrao virginianus.

other

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other serpents. Hence, too, upon opening the stomachs of some of our serpents, if we often find that they contain birds, it is almost entirely those birds which build in the manner I have just mentioned.

This fact I had long remarked. It had made some impression upon my mind before I had turned my attention to the subject of this memoir. Lately, when I came to take a view of the subject, the fact appeared to me to be of some consequence. I shall now avail myself of it.

The rattle-snake seldom, if ever, climbs up trees*. He is

* Some respectable writers assert, that the rattle-snake does climb trees, and that it does it with ease. Mr. de la Cépède is of this opinion. After telling us that this reptile lives upon worms, frogs, and hares, this naturalist proceeds: "il fait aussi sa proie d'oiseaux & d'écureuils; car il monte avec facilité sur les arbres, & s'y élance avec vivacité de branche en branche, ainsi que sur les pointes des rochers qu'il habite, & ce n'est que dans la plaine qu'il court avec difficulté, & qu'il est plus aisè d'eviter sa poursuite." *Histoire Naturelle des Serpens*. p. 409. At the conclusion of his account of the boiquira, or crotalus horridus, the eloquent author has run into the same error, in the following beautiful, though rather poetical, apostrophe. "Tranquilles habitans de nos contrées tempérées, que nous sommes plus heureux, loin de ces plages où la chaleur & l'humidité regnent avec tant de force! Nous ne voyons point un Serpent funeste infecter l'eau au milieu de laquelle il nage avec facilité; les arbres dont il parcourt les rameaux avec vitesse; la terre dont il peuple les cavernes; les bois solitaires, où il exerce le même empire que le tigre dans ses deserts brûlans, dont l'obscurite livre plus sûrement sa proie à sa morsure. Ne regrettons pas les beautés naturelles de ces climats plus chauds que le nôtre, leurs arbres plus touffus, leurs feuillages plus agréables, leurs fleurs plus suaves, plus belles: ces fleurs, ces feuillages, ces arbres cachent la demeure du Serpent à sonnette." *Histoire Naturelle des Serpens*. p. 419 & 420. I have been at some pains to discover whether the rattle-snake does climb up trees. The result of my inquiries is that it does not. Although I have had opportunities of seeing great numbers of rattle-snakes in the western parts of Pennsylvania, &c. particularly in the vicinity of the river Ohio, I never saw one of them except on the ground. The black-snake I have often seen upon trees. I ought not, however, to conceal that in the summer of the last year, a Choktah-Indian told me, that the rattle-snake does climb trees and bushes, *to a small height*. He said, that he had once seen one of these snakes upon a reed. I am not very willing

is frequently, however, found about their roots, especially in wet situations. It is said that this reptile is often seen, curled round a tree, darting terrible glances at a squirrel, which after some time is so much influenced by these glances, or by some subtile emanation from the body of the serpent, that the poor animal falls into the jaws of its enemy. This story is, I believe, destitute of foundation, though it is related by the good Cotton Mather*. The rattle-snake is, indeed, sometimes seen at the root of a tree, upon the lower branches of which, at the height of a few feet from the ground, a bird or squirrel has been seen exhibiting symptoms of fear and distress. Is this a matter of any wonder? Nature has taught different animals what animals are their enemies; and although, as will be afterwards shewn, the principal food of the rattle-snake is the great frog, yet as he occasionally devours birds and squirrels, to these animals he must necessarily be an object of fear. When the reptile, therefore, lies at the foot of a tree, the bird or the squirrel will feel itself uneasy. That it will sometimes run towards the serpent, then retire, and return again, I will not deny. But that it is irresistibly drawn into the jaws of the serpent, I do deny: because it is very frequently seen to drive the serpent from its hold; because the bird or squirrel often returns, in a few minutes, to their

Facts and observations respecting the power of fascination ascribed to snakes.

willing to deny this Indian's story: yet it is opposed to every information I have been able to procure from persons well acquainted with the reptile of which I am speaking. However, it is not impossible that where trees and bushes grow very close together, the snake may climb them *to a very small height.* Most species of serpents move in a spiral manner: the rattle-snake moves strait on; and this is the reason why he cannot climb trees. In the quotation which I have made from Mr. de la Cépède, another mistake is involved. He speaks of the agility with which the rattle-snake moves. This is not, however, merely the mistake of Mr. de la Cépède. We find it in Piso. Speaking of this reptile, our author says: "In triviis juxta ac deviis locis cernitur, tam celeriter proreptans ut volare videatur, idque velocius per loca saxosa, quam terrestria." *De India utriusque re naturali et medica.* p. 274. Now the truth is that the rattle-snake is one of the most sluggish of all our serpents. Linnæus was well informed, when he asserted that Providence had given "the Crotalus a very slow motion." See *Reflections, &c.* quoted p. 84 of this memoir.

* Philosophical Transactions of the Royal Society, No. 339.

habitations

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habitations. Sometimes the bird or squirrel, in attempting to drive away the snake, approach too near to their enemy, and are bitten, or immediately devoured. But, from what will afterwards be said, it will appear that these instances are not so common as is generally imagined.

My inquiries concerning the season of the year, at which any particular species of birds has been seen under the fascinating power of a serpent, afforded me still more satisfaction. In almost every instance, I found that the supposed fascinating faculty of the serpent was exerted upon the birds at the particular season of their laying their eggs, of their hatching, or of their rearing their young, still tender, and defenceless. I now began to suspect, that the cries and fears of birds supposed to be fascinated originated in an endeavour to protect their nest or young. My inquiries have convinced me that this is the case.

I have already observed, that the rattle-snake does not climb up trees. But the black-snake and some other species of the genus coluber do. When impelled by hunger, and incapable of satisfying it by the capture of animals on the ground, they begin to glide up trees or bushes, upon which a bird has its nest. The bird is not ignorant of the serpent's object. She leaves her nest, whether it contains eggs or young ones, and endeavours to oppose the reptile's progress. In doing this, she is actuated by the strength of her instinctive attachment to her eggs, or of affection to her young. Her cry is melancholy, her motions are tremulous, She exposes herself to the most imminent danger. Sometimes, she approaches so near the reptile that he seizes her as his prey. But this is far from being universally the case. Often, she compels the serpent to leave the tree, and then returns to her nest *.

* Horace, though he has not, like his contemporary, Virgil, given any great proofs of his knowledge in natural history, appears to have known, full well, the anxiety of birds for the preservation of their young:

"Ut assidens implumibus pullis avis
"Serpentium allapsus timet."

Epod. 1.

The author of these two fine lines, had he lived in America, the land of fascination, would, I am inclined to think, have disbelieved, the whole story. They would have been a clue to light and truth on this subject.

It

Facts and observations respecting the power of fascination ascribed to snakes.

It is a well known fact, that among some species of birds, the female, at a certain period, is accustomed to compel the young ones to leave the nest; that is, when the young have acquired so much strength that they are no longer entitled to *all* her care. But they still claim some of her care. Their flights are awkward, and soon broken by fatigue. They fall to the ground, where they are frequently exposed to the attacks of the serpent, which attempts to devour them. In this situation of affairs, the mother will place herself upon a branch of a tree, or bush, in the vicinity of the serpent. She will dart upon the serpent, in order to prevent the destruction of her young: but fear, the instinct of self-preservation, will compel her to retire. She leaves the serpent, however, but for a short time, and then returns again. Oftentimes, she prevents the destruction of her young, attacking the snake, with her wing, her beak, or her claws. Should the reptile succeed in capturing the young, the mother is exposed to less danger. For, whilst engaged in swallowing them, he has neither inclination nor power to seize upon the old one. But the appetite of the serpent-tribe is great: the capacity of their stomachs is not less so. The danger of the mother is at hand, when the young are devoured. The snake seizes upon her: and this is the catastrophe, which crowns the tale of fascination!

An attachment to our offspring is not peculiar to the human kind alone. It is an instinct which pervades the universe of animals. It is a spark of the divinity that actuates the greater number of living existences. It is a passion which, in my mind, at least, declares, in language most emphatic, the existence, the superintendance, the benevolence, of a first great cause, who regards with partial and parental, if not with equal eyes the falling of a sparrow and the falling of an empire.

Among the greater number of the species of birds, the attachment of the parent to the young is remarkably strong. We have daily instances of this attachment among our domestic birds, and I believe, it is stronger among these birds in their wild state: for there are some reasons for suspecting, that this amiable instinct is diminished and weakened by culture *.

* This question will be examined in my memoirs upon the storge, or affections, of animals.

The

Facts and observations respecting the power of fascination ascribed to snakes.

The instances which I have already mentioned, as well as a fact, which remains to be mentioned, point out, in a striking view, the attachment of the mother-bird to her offspring. She often guards her nest with the greatest attention, fearful of the insidious glide of the serpent. She endeavours to prevent the destruction of her eggs or young, by this enemy. When he has succeeded in obtaining them, she attacks him either alone, or calls other birds to her assistance. We ought not to be surprised, that sometimes she falls a victim to her affection. For it is a well known fact, that some species of birds will suffer themselves to be taken upon their nests, rather than relinquish their young, or their eggs.

In the study of natural history, I am always happy to discover new instances of the wisdom of providence, and new proofs of the strong affections of animals. And for the discovery of such instances of wisdom, and such proofs of affection, the contemplation of nature is an ample field. In the instances now before us, the strength of the instinct of affection in birds is illustrated, in a striking point of view; and I cannot help observing, that I feel an high degree of pleasure in being able to do away, in some measure at least, a prejudice, not less extensive than it is unfounded, by bearing my slender testimony in favour of the existence and the powerful dominion of a benevolent principle in animals.

The following fact was communicated to me, some time since, by our president, Mr. Rittenhouse. I think it strikingly illustrates and confirms the system which I have been endeavouring to establish. I relate it, therefore, with pleasure, and the more so, as I have no doubt, that the authority of a cautious and enlightened philosopher will greatly contribute to the destruction of a superstitious notion which disgraces the page of natural history.

Some years since, this ingenious gentleman was induced to suppose, from the peculiar melancholy cry of a red-winged maize-thief*, that a snake was at no great distance from it, and that the bird was in distress. He threw a stone at the place from which the cry proceeded, which had the effect of driving the bird away. The poor animal, however, im-

* Commonly called, in Pennsylvania, the Swamp-Black-bird. It is the Oriolus phœniceus of Linnæus.

mediately

mediately returned to the same spot. Mr. Rittenhouse now went to the place where the bird alighted, and, to his great astonishment, he found it perched upon the back of a large black-snake, which it was pecking with its beak. At this very time, the serpent was in the act of swallowing a young bird, and from the enlarged size of the reptile's belly it was evident, that it had already swallowed two or three other young birds. After the snake was killed, the old bird flew away. Facts and observations respecting the power of fascination ascribed to snakes.

Mr. Rittenhouse says, that the cry and actions of this bird had been precisely similar to those of a bird which is said to be under the fascinating influence of a serpent; and I doubt not that this very instance would, by many credulous persons, have been adduced as a proof of the existence of such a faculty. But what can be more evident than the general explanation of this case? The maize-thief builds its nest in low bushes, the bottoms of which are the usual haunts of the black-snake. The reptile found no difficulty in gliding up to the nest, from which, most probably in the absence of the mother, it had taken the young ones. Or it had seized the young ones, after they had been forced from the nest, by the mother. In either case, the mother had come to prevent them from being devoured.

We are well acquainted with the common food of the rattle-snake. It is the great-frog * of our rivers, creeks, and other waters. The snake lies insidiously in wait for his prey, at the water-edge. He employs no machinery of enchantment. He trusts to his cunning and his strength.

A very ingenious † friend of mine, who has devoted considerable attention to the natural history of the rattle-snake, and who has dissected many of them, assures me, that he never saw but one instance in which a bird was found in the stomach of this reptile, and this bird was the chewink, or ground-robin ‡. In another instance, he saw a ground-squirrel § taken out of one of these reptiles. In every other case, so long as the food retained enough of the form to be distinguished, the stomach was found to contain the great-frog, which I have mentioned.

* Rana ocellata of Linnæus. † Timothy Matlack, Esq.

‡ This is the Fringilla erythrophthalma of Linnæus.

§ The Sciurus striatus of Linnæus.

Another

Facts and obſervations reſpecting the power of faſcination aſcribed to ſnakes.

Another argument againſt the faſcinating power of the ſerpent-tribe ſtill remains to be conſidered.

It is natural to inquire, for what purpoſe nature has endued ſerpents with the ſuppoſed powers of faſcinating birds, and other animals? The anſwer to this queſtion is uniform. It is ſaid, the power is given that the ſerpents may obtain their food. Let us examine this opinion.

Admitting the exiſtence of this power, I ſhould have no heſitation in believing, that its uſe is what is here mentioned, though, indeed, it ought not to be concealed, that ſnakes are ſuppoſed, by ſome fooliſh people, to have the power of charming even children. And yet, I believe, there are no inſtances recorded of our American ſnakes devouring children. If, then, nature, in the immenſity of her kindneſs, had gifted the ſerpents with this wonderful power, we ſhould, at leaſt, expect to find that the common and principal food of theſe ſerpents was thoſe animals, viz. birds and ſquirrels, upon which this influence is generally obſerved to be exerted. This, however, is by no means the caſe.

As connected with this part of my memoir, it will not be improper to obſerve, that all our ſerpents are the food of different kinds of birds. Even the rattle-ſnake, whoſe poiſon produces ſuch alarming ſymptoms in man, and other animals, is frequently devoured by ſome of our ſtronger and more courageous birds. As far as I can learn, the birds which moſt commonly attack and deſtroy this reptile, are the ſwallow-tailed hawk *, and the larger kinds of owls. The owl often feeds her young with this ſnake, whoſe bones are frequently found in her neſt, at conſiderable heights from the ground. Even a hen has been known to leave, for a minute, her affrighted chickens, and attack, with her beak, a rattle-ſnake, the greater part of whoſe body ſhe afterwards devoured †.

* Falco furcatus.

† It is commonly believed, that the rattle-ſnake is a very hardy animal; but this is not the caſe. A very ſmall ſtroke on any part of its body diſables it from running at all; and the ſlighteſt ſtroke upon the top of the head is followed by inſtant death. The ſkull-bone is remarkably thin and brittle; ſo much ſo indeed, that it is thought that a ſtroke from a wing of a thruſh or robin would be ſufficient to break it.

The

The black-snake is a serpent of much more activity than the rattle-snake. The latter, as I have already said*, seldom, if ever, climbs up trees. But the former will sometimes ascend the loftiest trees, in pursuit of the object of his appetite. The rattle-snake, it has been just observed, subsists principally upon the large frog, which frequents the waters of our country. He has, therefore, but little occasion for activity. But the black-snake, feeding more upon birds, stands more in need of activity. He frequently glides up the trees of the forest, &c. and, commonly in the absence of the mother, devours either her eggs or her young ones. The difficulty of obtaining his prey upon the tree is sometimes very considerable, as will appear from a fact which will be related immediately. Now, if this serpent is gifted with the faculty of fascinating, why is he not content to continue at the bottom of the tree, and bring down his object? And if he can employ this machinery of fascination at his pleasure, how comes it, that he so seldom succeeds in capturing old birds? For it is a fact that when birds are found in his stomach, they are principally young birds.

Facts and observations respecting the power of fascination ascribed to snakes.

I have said, that the black-snake sometimes finds great difficulty in obtaining his prey upon the tree. In support of this assertion I could adduce many facts. But my memoir has already exceeded the limits which I originally prescribed to it. I shall content myself, therefore, with relating a solitary fact, which strikingly illustrates my position.

A black-snake was seen climbing up a tree, evidently with the view of procuring the young birds in the nest of a baltimore-bird. This bird, it has been already observed, suspends its nest at the extremity of the branch of a tree. The branch to which the bird, of which I am speaking, had affixed its nest, being very slender, the serpent found it impossible to come at the nest by crawling along it: he, therefore, took the advantage of another branch, which hung above the nest, and twisting a small portion of his tail around it, he was enabled, by stretching the remainder of his body, to reach the nest, into which he insinuated his head, and thus glutted his appetite with the young birds.

The importance of this fact, in the investigation of the subject of my memoir, appears to me to be great. An

* See page 106.

Facts and observations respecting the power of fascination ascribed to snakes.

American forest is not the silent residence of a few birds. During the greater part of the spring and summer months, our woods are alive with the numerous species of resident and visitant birds. At these times, if the black-snake possesses the faculty of fascinating, it cannot be a difficult thing for him to procure his food. Yet, in the instance which I have just related, we have seen this reptile climbing up a tree, and there obliged to exert all his ingenuity to obtain his prey.

I cannot well conclude this memoir without observing, that in the investigation of the subject which it involves, I have experienced much pleasure. For to the cultivators of science, the discovery of truth must, at all times, be a source of pleasure. This pleasure will even rise to something like happiness, when, in addition to the discovery of truth, we are enabled to draw aside the veil, which, for ages, has curtained superstition and credulity. Under the influence of various species of superstition, we fall from our dignity, and are often rendered unhappy. It should be one of the principal objects of science to rear and prop the dignity of the mind, and to smooth its way to comforts, and to happiness. The ills and the infirmities of our earthly state of being are numerous enough. It is folly, if not vice, to increase them. He who seriously believes, that an hideous reptile is gifted, from the sacred source of universal life and good, with the power of fascinating birds, squirrels, and other animals, will hardly stop here. He may, and probably will, believe much more. He will not, perhaps, think himself entirely exempted from this wonderful influence. He may suppose, that the property belongs to other beings, besides the serpents; and he will, perhaps, imagine that it forms a part of a more extensive plan, the effects of which, he will affert, are prominent, and unequivocal, though its ways, he will confess, are incomprehensible to mortal minds.

Historia naturalis non bene digesta abit in fabulam; prejudicia vero et nimia credulitas Veritatem; etsi cominus satis cognitam, longissime aliquando propellunt.

JACOBUS THEODORUS KLEIN.

Some

XV.

Some Account of an Egyptian Lock of very high Antiquity. Indicated from DENON'S *Travels, by a Correspondent. With Observations by* W. N.

To Mr. NICHOLSON.

SIR,

A FEW weeks ago I saw, with particular pleasure, a model, or wooden lock, made from the description in Denon's travels, which was exhibited in the lectures at the Royal Institution. I beg leave to propose the insertion of the same in your excellent Work, and should hope for your opinion as to its security.

Letter concerning a lock shewn at the Royal Institution.

I am, Sir,

Your obliged reader,

P. Q.

New Broad Street,
May 12, 1804.

I HAVE given engravings of this lock, copied from Denon's book. *Plate* VII, shews the lock as applied to a door, and its developement is made in *Plate* VIII. The passage in Denon (translated) is as follows:

Description of the lock,

"No. 2, 3, 4, and 6. The Egyptian lock. It secures the gates of towns, of houses, and the apertures of the smallest articles of furniture or use. I have placed it among the antiquities, because it is the same as was in use four thousand years ago. I found one sculptured among the bas-reliefs which decorate the great temple of Karnack. It is simple in conception, easy of execution, no less sure than any other lock, and deserves to be applied on all our rural occasions. Fig. 2 is the key, which is capable of thousands of different combinations. In Fig. 3 the lock is shewn closed, seen as to its interior; the key being in the act of lifting up certain pins which had fallen into holes in the bolt, and kept it in its place. In Fig. 1 the bolt is drawn back, and the lock consequently open." *See plate 139 of the said work.*

sculptured on the great temple of Karnack.

Eton, in his Survey of the Turkish Empire, mentions this lock as follows:

"Nothing can be more clumsy than the door-locks in Turkey, but their mechanism to prevent picking is admirable. It

Eton's account of the same lock as of universal use in Turkey, &c.

It is a curious thing to see wooden locks upon iron doors, particularly in Asia, and on their caravanseries and other great buildings, as well as on house doors. The key goes into the back part of the bolt, and is composed of a square stick with five or six iron or wooden pins about half an inch long, towards the end of it, placed at irrregular distances, and answering to holes in the upper part of the bolt, which is pierced with a square hole to receive the key. The key being put in as far as it will go, is then lifted up, and the pins entering corresponding holes raise other pins, which had dropt into these holes from the part of the lock immediately above, and which have heads to prevent them falling lower than is necessary. The bolt being thus freed from the upper pins, is drawn back by means of the key, the key is then lowered, and may be drawn out of the bolt; to lock it again the bolt is only pushed in, and the upper pins fall into the holes of their own weight. This idea might be improved on, but the Turks never think of improving."

Observations.

THE simplicity and other advantages of this lock or bolt, are too obvious to require much remark; for which reason I shall confine my present observations to its degree of security or inviolability. I think we may contemplate it in three several stages of perfection or improvement.

Simplest structure of this lock is a kind of latch.

1. If it be constructed with one pin of considerable size to fall into the bolt, and the finger be supposed to be introduced for the purpose of raising it and setting the bolt at liberty, we shall have a fastening of nearly the same effect as the common latch.

The present lock.

2. Or if, instead of one falling pin, there be many, and an instrument be used to lift them, we shall have the bolt before us: and this, as far as we are informed, is the present state of the invention, though of so long standing.

Probable improvement.

3. Or thirdly, in case the present bolt should, on examination, be found to admit of being opened without extreme difficulty, it will become a question whether the principles of its structure can be so applied as to render it absolutely safe. This last question requires that we should first examine the ancient bolt a little more closely.

The lock cannot be picked;

We may admit that the ancient lock, with many pins falling independently of each other, cannot be picked or opened without its key; and therefore we must ask whether the key can

can be made from an examination of the lock alone? In answer to this it may be noticed, that since all the pins in the key must be of equal height, the secret will consist in their relative distances and positions on the face of the key; and that these distances and positions can be easily known by introducing a stick, or key without pins, into the hollow of the bolt, and taking an impression (by means of a facing of soft clay) of the holes intended to receive the pins. After this a key may be made without difficulty; so that we arrive at our conclusion, that though this ingenious piece of mechanism cannot be violated without its key, yet it is easy to construct a key for that purpose.

but its key may be easily made from the lock.

We now come to the enquiry after that application of its principles which may render it absolutely safe. This is certainly possible; but not without considerably diminishing its simplicity. Besides several others, the following may be proposed: Let the dropping pin have an enlarged part, and a tail of wire above and below. Let the lower tail fall into its hole in the bolt, while the enlarged part falls into a socket made for its reception. Under these circumstances the bolt becomes fast: But when by raising the pin the enlarged part is clear of its socket, the bolt becomes free, and the lower tail is prevented from stopping it by a groove left for its reception. The bolt must have a covering piece of board, having a hole of the size of the enlarged part of the pin, directly above the socket into which it falls, and a groove for its upper tail; the interval between the covering-piece and the bolt itself being equal to the height of the enlarged part of the pin. By this means, when the pin is pushed up, just out of its socket, the bolt will move freely; but if it be pushed the least quantity farther, the enlarged part will enter the hole in the covering-board, and set it fast, as if it were in the socket; so that a very precise distance of elevation will be requisite. Lastly, the lower pin may be shorter or longer at pleasure. Now, if there be a number of these pins so placed and adjusted as to fall into their respective sockets at the position of the bolt when shut; if their lower tails be of different lengths, and a key be made to correspond with them, and lift them all to the proper height at once; the combination will be such as cannot be made out by any impression or tentative process upon the lock itself. For the evidence of a due length of any

Improvement by which it is rendered safe.

4 one

one of the lifting pieces of the key, will confift in the actual opening of the lock; and this cannot be had unlefs the due length of all the pins be obtained at once; againft which the probability will be as the number of permutations of the pins, multiplied into the number of poffible lengths of pins practically differing from each other in effect. Thus if the pins were fix, the permutations would be $1\times2\times3\times4\times5\times6=720$; and if the length of a whole pin were one quarter of an inch, and a fenfible difference would in practice arife from making the pins one-fixtieth of an inch longer or fhorter, there would be 15 poffible lengths for every pin. Whence $720\times15=10800$, the number of chances againft the difcovery.

W. N.

XVI.

On the Caufe of the different Colours of the Triple Sets of Platina, and on the Exiftence of a new Metallic Subftance in that Metal. By COLLET-DESCOTILS.* *Prefented to the Clafs of Mathematical and Phyfical Sciences of the National Inftitute of France.*

Precipitation of the folution of platina.

ALL chemifts know that crude platina is eafily foluble only in nitro-muriatic acid, that the folution is decompofable by muriate of ammonia and other falts with alkaline bafes; and that the refult of their former decompofition is a triple compound, confifting of oxid of platina, muriatic acid, and ammonia, or the alkali employed. The colour of this precipitate varies from a light yellow to a dark brown. It is fometimes alfo greenifh. The latter is the cafe if the folution of platina is precipitated by a falt with bafe of foda.

Remarks.

Before I fay any thing further concerning the caufes which influence the colour of this precipitate, I fhall point out fome phenomena which characterife the folution of the metal itfelf.

Foreign admixtures in crude platina.

The grains of platina of commerce always contain more or lefs of foreign mixtures from which it fhould be previoufly freed as much as poffible. The foreign bodies met with are moft frequently minute ftones, on which the acid employed for diffolving the metal, has little or no action, and two forts of ferrugi-

* Gehlen's Chemical Journal, Vol. II. Part I. p. 73.

nous

nous ſand, one of which is obedient to the magnet, and another which is not attracted by the magnet, and which is only partially acted on by acids. I ſhall ſay no more in this place concerning theſe bodies, but that the firſt contains titanium and the ſecond chromic acid, in conſiderable quantity.

The ferruginous ſands contain titanium and chromic acid.

The beſt method to free platina of commerce from theſe admixtures, is that recommended by Prouſt, which conſiſts in ſpreading out the platina on a ſheet of paper, and carefully blowing away the lighter parts by means of a pair of bellows.

Mechanical purification by bellows.

Platina thus purified I introduced into a porcelain retort, to which a glaſs receiver had been fitted, previouſly filled one-third full of water. After having placed the retort in a reverberatory furnace, I raiſed the fire gradually, and increaſed the heat to the utmoſt I could produce, which was kept up for two hours. Nothing particular attended this proceſs, except that the water with which the receiver had been partly filled, acquired a greeniſh hue towards the end of the proceſs. On the roof of the retort, a fine blue powder was ſublimed, of which I ſhall ſay no more at preſent than that it was ſoluble in water. The water in the receiver, after having been ſuffered to ſtand for a few days, had acquired a beautiful blue colour, which ſurpaſſed the colour of the beſt ultramarine.

Crude platina by violent diſtillation affords a blue ſublimate ſoluble in water.

It was impoſſible to get the platina out of the retort. On breaking the diſtillatory veſſel, the metal was found aglutinated, the upper ſurface of the maſs had a ruſty appearance, the middle was leſs diſcoloured, and the lower part had ſuffered no perceptible change.

The platina grains were agglutinated.

In order to examine the colouring matter which tinged the water of the receiver, I dropt into it a ſolution of an alcali; this produced inſtantly a blue precipitate. Sulphuric and muriatic acids, when mingled with this fluid, occaſioned no change. Nitric and oxiginized muriatic acids changed this blue fluid, firſt to a lilac, but it ſoon loſt this colour, and the whole became limpid. Water holding in ſolution ſulphuretted hydrogen gas, occaſioned no precipitate; but hidro-ſulphuret of ammonia threw down a grey precipitate, which became blue by the affuſion of acids, and then was rendered ſoluble again in hidro-ſulphuret of ammonia.

The aqueous ſolution of the blue matter afforded a precipitate by alcali. Habitudes with acids, &c.

A ſmall quantity of the blue precipitate collected from the roof of the retort, when urged with the blow-pipe in contact with borax, imparted no colour to this ſalt. When heated *per ſe*, it diſappeared completely.

The blue ſublimate diſappeared before the blow-pipe: it did not colour borax.

Having

When platina is diſſolved in n. m. acid, a black powder is ſeparated.

Having ſo far proceeded, I made a ſolution of platina in nitro-muriatic acid (the platina had been freed from iron as much as poſſible by muriatic acid.) During this ſolution, a glittering black powder became ſeparated, as is always the caſe when platina of commerce is diſſolved in nitro-muriatic acid. If the operator be careful in collecting this powder as faſt as it is ſeparated, the quantity which may be collected, amounts to about 0,03 of the platina employed. But if this powder be not removed as faſt as it is depoſited in the ſolution, part of it becomes again acted upon, and a much leſs quantity is obtained.

Muriate of pl. was decompoſed by mur. of ammonia.

The muriate of platina obtained, after having been ſuffered to repoſe and being decanted, I decompoſed by adding by a ſaturated ſolution of muriate of ammonia; the precipitate was ſeparated by decanting the ſupernatant fluid, and repeatedly waſhed till the water which paſſed, did not become green by the addition of pruſſiate of potaſh. The precipitate obtained was of *a yellow colour*. The decanted fluid from which the precipitate had been ſeparated, and the firſt quantity of water employed for its ablution, after having been mingled together and concentrated by evaporation to one third of its bulk, were again mixed with a ſolution of muriate of ammonia; the precipitate now obtained was of a *dark red colour*. On treating the fluid ſeparated from it, as before, the precipitate which did fall down was of a very *dark brown*. All theſe different coloured precipitates were carefully waſhed till they contained no veſtige of copper or iron.

Yellow precipitate.

The decanted fluid being concentrated, and more muriate of amm. added, a *dark red* precipitate was obtained.

The fluid à ſecond time decanted gave by mur. amm. a dark brown precip.

I have remarked that if the ſolution of platina be ſlowly prepared, that is to ſay, if the platina be introduced into the nitro-muriatic acid, in ſmall quantities at a time, and the ſubſequent ſolution of each quantity be reſpectively ſeparated, and ſeparately be decompoſed by muriate of ammonia, the colour of the precipitate or triple ſalt obtained, is darker in proportion to the quantity of the black powder which was contained in the ſolution.

The colour of the precip. of platina is darker the more of the black powder in the ſolution.

The black powder is (difficultly) ſoluble in nitr. m. acid; and decomp. by mur. of am.

This black powder which is depoſited during the ſolution of platina of commerce * in nitro-muriatic acid, is ſoluble (though difficultly) in nitro-muriatic acid, compoſed of much nitric, and little muriatic acid; its ſolution is alſo decompoſable by

* This black powder is likewiſe ſeparated during the ſolution of *malleable platina* in nitro-muriatic acid. F. A.

muriate

muriate of ammonia, and the colour of the precipitate is more or lefs intenfe, according to the quantity of powder contained in the folution.

From what has been fo far ftated, it appears, that this black powder is the caufe of the different colours which the different precipitates or triple falts of platina exhibit, under different circumftances.

The black powder is the caufe of colour in the precipitates.

In order to learn the nature of this fubftance, I fhall detail the experiments which were undertaken for that purpofe. The precipitates or falts I made ufe of were, the triple ammoniacal muriate of platina, and the triple muriate of platina and foda; the former falt I preferred on account of its eafy decompofibility, and the latter on account of being very foluble.

Enquiry into the nature of this fubftance.

Experiments on the triple ammoniacal Muriate of Platina.

Equal quantities of the before-obtained yellow and dark red precipitate, being feparately diffolved in equal quantites of water, the firft falt furnifhed a folution of a gold yellow colour, whereas that of the latter was orange red. On adding to the latter folution a minute quantity of green fulphate of iron, or fulphureous acid, it became inftantly of a gold yellow colour; the fame effect was produced, though flowly, by the addition of alcohol.

Aqueous folutions of the yellow and dark red precipitates; the firft gold-yellow, the fecond orange red.

The latter was rendered yellow by a fm. portion of gr. fulph. of iron, or by alcohol.

It was natural to fuppofe that the colour of the red falt might be owing to the higher degree of oxidizement of the diffolved platina. In order to convince myfelf of this conjecture, I attempted to transfer oxigen to the yellow falt, by means of nitric and oxiginized muriatic acids. This however failed; the colour of the falt remained yellow as before. On repeating the application of nitric and oxiginized muriatic acids alternately, the refult was only a very pale red coloured precipitate.

The red colour was not owing to greater oxidizement.

Equal parts of the yellow and red precipitate, deficcated at equal temperatures, and under the fame circumftances, when decompofed by heat, yield alfo unequal quantities of fixed refidue. That of the firft, amounted to 0,44 of the weight of the falt employed, and that of the latter was 0,4 and 5.

The yellow and red precipitates do not leave equal refidues, when decompofed by heat.

If yellow precipitate be reduced by heat, the platina obtained is *uncommonly foluble* in a comparatively fmall quantity of nitro-muriatic acid, and the folution yield a yellow precipitate with muriate of ammonia.

The yellow precipitate if reduced, affords very foluble platina; which again affords a yellow precip.

If

The red precipitate affords a less soluble metal which leaves a black powder, and affords a red precip.

If the red precipitate be reduced, the metal obtained is of a different nature from the former. It is far more insoluble in nitro-muriatic acid, and whatever quantity of acid may be applied, there remains constantly a quantity of black powder, which is absolutely insoluble in the acid. This solution, on being decomposed by muriate of ammonia, yields a red precipitate.

This precipitate treated with oxiginized muriate of potash with heat.

To learn the nature of this precipitate, I reduced a quantity of it, introduced it into a porcelain tube, connected with a small retort, containing hyper-oxiginized muriate of potash, and applied heat to the retort; after having first adapted to the other extremity of a receiver containing a little water. On increasing the heat so as to decompose the hyper-oxiginized muriate, the tube became lined with a blue powder, which was also observed in the empty part of the receiver.

Blue powder obtained;

After all the salt in the retort had been decomposed, I collected the blue sublimed powder. The minute quantity of it however did not permit me to submit it to many experiments. It was easily soluble in nitro-muriatic acid. Its nature will become more obvious hereafter.

—soluble in nitro-muriatic acid.

The red colour arises from a peculiar metal.

From what has been so far stated, it appears to follow that the red colour of the triple precipitate of platina, obtained by muriate of ammonia, or other salts with alkaline bases, is owing to the presence of a peculiar metal contained in the platina, which has hitherto been considered as simple.

Examination of the triple Muriate of Platina and Soda.

Triple muriate of platina and soda.

This triple compound is very little known.* It may easily be obtained by pouring into a solution of platina, a salt with base of soda. It is very soluble in water, and even in alcohol. The solutions are capable of crystallizing in long prisms, on three-sided tables, of a yellowish red colour. It is decomposable by muriate of ammonia; the precipitate is a muriate of platina and ammonia. It is likewise decomposed by a solution of soda; on adding this alcali in excess, the formed precipitate becomes again dissolved.

Reducible on charcoal, by the blow-pipe.

Muriate of platina and soda is reducible upon charcoal before the blow-pipe. The reduced metal possessed a considerable lustre.

* Mussin-Puschkin has pointed out some of its properties in Crell's Annales, 1800, Vol. I. p. 93. of which a short abstract is to be found in the Annales de Chimie. p. 277.

If cryſtallized muriate of platina and ſoda, free from all adhering or exceſs of acid, be expoſed for ſome time to the air, its beautiful red colour becomes changed into a greeniſh hue. If the ſalt in that ſtate be diſſolved in water, and oxiginized muriate of lime be added to it, a dark brown precipitate falls, which after having been waſhed and dried, is ſoluble in muriatic acid, with which it forms a *beautiful blue ſolution.* This colour becomes again deſtroyed by the admixture of alcohol, and re-appears by the addition of oxiginized muriate of lime.

Its cryſtals from a beautiful red, become green by expoſure to the atmoſphere. Its ſolution precip. by ox. muriate of lime.

This precipitate is ſomewhat ſoluble in water; it is reducible when fuſed with borax, without imparting to the latter any colour. The reduced metal is very porous: it appeared not to be ſoluble in any of the acids.

Precipitate ſomewhat ſoluble; reducible into a porous metal inſoluble in acids.

If the ſolution of muriate of platina and ſoda, contain an exceſs of acid, it then is not diſturbed by letting fall into it oxiginized muriate of lime; but on evaporation the mixture acquires a fine *green colour.*

No precipitate if acid be in exceſs.

Examination of the Yellow and Red triple Muriate of Platina.

If we add to the red triple muriate of platina, a ſolution of carbonate of ſoda, till it becomes completely diſſolved the ſolution is of a yellow colour. On expoſing it for ſome time to the contact of air, a green ſubſtance becomes depoſited.

The red triple muriate becomes yellow by carbonate of ſoda; —and depoſits a green precip. by expoſ. to the air.

The yellow triple muriate of platina, treated in a like manner with carbonate of ſoda, yields a ſolution of a dark yellow, or orange colour, which ſuffers no change whatever on expoſure to air.

The yellow thus treated becomes darker, and does not afterwards change.

The ſeparation of the green ſubſtance from the red triple muriate, is much accelerated by the admixture of oxiginized muriatic acid. It ſeems as if the ſeparation of this ſubſtance is owing to the action of oxigen; for no other acid is capable of producing it; at leaſt the precipitate which they produce, is a mere oxid of platina.

The green depoſit from the red triple muriate is from oxidizement.

The green ſubſtance may likewiſe be inſtantly obtained by evaporating the ſolution by heat.

It is inſtantly had by evaporation.

If the ſolution of the yellow triple muriate be heated and evaporated, a very minute quantity of the green ſubſtance is indeed, ſometimes depoſited, and the remaining ſolution then acquires a more beautiful yellow colour. On continuing the evaporation of the yellow ſolution, a precipitate is depoſited,

The yellow triple muriate may afford a minute portion of green precip. by evap.—but this is an impurity.

which

Its precip. is yellow.

which is not green, but *yellow*. This precipitate on being again dissolved in muriatic acid, and saturated with carbonate of soda, yields no farther green substance.

The green precipitate was reduced (by heat with borax) to a white brittle metal; difficultly acted on by nitro-m. acid;

I mingled a quantity of this green substance with vitrified borate of soda, and exposed the mixture for twenty-five minutes in a double crucible to the most intense heat I could produce; the result was a white brittle metal, which was difficultly acted upon by nitro-muriatic acid. The weak solution which had been obtained by this acid, had a violet colour; it yielded, on being evaporated, a dark green residue, which was soluble in muriatic acid, with which it formed a green fluid.

more so when powdered.

On pulverizing another quantity of this white brittle metal, and then exposing it to the action of nitro-muriatic acid, the solution was effected more easily, it was now more concentrated, and of a reddish-yellow colour. Muriate of ammonia let fall into it, threw down a little brownish-red precipitate, a proof that this metal still contained platina.

The solution of the green substance became yellowish by sulphureous acid or green muriate of iron.

A quantity of the before-mentioned green substance being dissolved in muriatic acid, acquired a yellowish colour on being mingled with sulphureous acid, or with a solution of green muriate of iron. Oxigenized muriate of lime restored the original green colour.

Ox. mur. of lime restored its green.

The green substance gave a blue sublimate, and left metal.

A third part of the green substance, on being exposed to a violent heat in a porcelain retort, yielded a blackish-blue sublimate: the unsublimed part was a metallic substance, very difficultly soluble in nitro-muriatic acid.

A portion of the same substance heated with nitre till the acid was decomposed. The alkali was dissolved in water, and not changed by acids. The residue was not acted on by acids. Hence chrome and molybdena were absent.

A fourth portion of the green substance was urged, in a crucible head, with nitrate of potash. After the decomposition of the nitrate had been completed, the mass was diffused through water. The alkaline solution was colourless, and acids produced no change of colour in it. The residue, after repeated ablutions, was hardly acted upon by acids, the nitro-muriatic acid not excepted. This experiment precludes the presence of chrome and molybdena.

Separation of the platina from the triple muriate by alcohol.

It is possible to separate nearly all the platina which is contained in the red triple muriate, by the mere admixture of alcohol, and the subsequent addition of dry potash or soda; for doing this much heat is evolved, and the reduction of platina takes place almost instantly. The same may be effected by means of carbonate of potash or soda; but in that case the solution must be highly concentrated. The reduction of the platina

platina then takes place even without the application of heat, but only after the ſpace of ſome days.

The ſupernatant fluid of the reduced platina, when heated, acquires a lilac colour, which becomes blue on long expoſure to air, depoſiting at laſt a greeniſh ſubſtance, reſembling that hitherto treated of. Oxigenized muriatic acid favours the ſeparation of this ſubſtance.

The ſupernatant fluid affords the blue ſubſtance.

Sulphuretted hidrogen may alſo be employed for ſeparating the platina from the red triple muriate; the platina becomes ſeparated in the form of a brown powder; the other metallic ſubſtance remains undiſturbed in the ſolution. It may, however, almoſt totally be precipitated by liquid ammonia. The precipitate obtained is brown. When fuſed with potaſh in a ſilver crucible, the maſs acquires a green colour: On pouring muriatic acid on it, no complete ſolution could be effected; there always remained a powdery ſediment which reſiſted likewiſe the action of nitro-muriatic acid. Carbonate of potaſh ſeparated from this ſolution a ſmall quantity of iron. The clear fluid from which the iron had been ſeparated, remained perfectly tranſparent when heated; but it acquired a bluiſh hue, which increaſed on concentration, and after the exſiccation of the ſalt. On adding a little nitric acid to the ſalt, the blue colour became changed into a dark red.

Sulphuretted hidrogen alſo ſeparates the platina.

CONCLUSION.

I now flatter myſelf with having proved, by the above experiments, that the red coloured triple ſalts of platina owe their colour to the preſence of a peculiar metal, oxidized to a certain degree.

Concluſion. The red colour is owing to a peculiar oxide,

That this metal is nearly wholly inſoluble in acids; that it becomes ſoluble in combination with platina; that when oxidized, it appears in the form of a blue oxide inclining to green; that its oxides, when combined with platina, are ſoluble in alcalies; that its acid ſolutions are not decompoſable by ſulphuretted hidrogen; that it imparts no colour to borax; that its oxides are reducible and volatilizable by heat, which volatilization becomes favoured by a ſtream of oxigen gas; and, laſtly, that oxigen gas, aſſiſted by heat, is capable of oxidizing this metal, and of volatilizing it in the form of a blue oxide.

nearly inſoluble in acids, unleſs combined with platina, &c.

These

The new metal not yet named.

These properties do not characterize any of the known metals, and I am therefore authorized to consider it as a new one, to which I shall give a name when I have explored its nature more fully.

XVII.

Description of an Apparatus for filtering Water. By Mess. HARMAN *and* DEARN, *of Redriff.*

Apparatus for filtering water.

THE waters which run near or upon the surface of the earth, are usually contaminated by the remains of animal and vegetable substances in their progress towards entire decomposition, as well as by the minute powder of earthy or mineral bodies, which render it turbid and less fit for the purposes of domestic life. Spring or pump waters, by a natural filtration through the sandy strata of the ground, are mostly cleared from these mechanical admixtures; but in many places, as is the case with those of our metropolis, they are rendered impure, or, as it is called, hard, by an actual solution of sulphate of lime or plaster of Paris, which prevents their lathering with soap, and probably renders them less wholesome; besides which, they usually carry a portion of the drainage water in great towns, which renders them offensive at certain seasons, and at all times less worthy of confidence. For these and numerous other reasons, it has always been considered as a desirable object to clear waters, by filtration, from those impurities which render them less limpid, and a variety of apparatus have been offered to the public for that purpose.

In all these the process of nature has been imitated; namely, by causing the water to percolate either through sand or a sand-stone; the latter of which, though costly, seems at present to be almost the only method in use among us.

The contrivers of the simple and cheap apparatus delineated in Plate VI. are Mess. Harman and Dearn, potters at Redriff, who remark, that the filtering-stone is not only expensive and liable to be clogged up and spoiled by the bodies deposited in its pores from the water, but that, as these bodies are actually in the progress to decay and decomposition, they are in some cases found actually to change the flavour and affect the purity of

of the fluid they are made use of to ameliorate. In consequence of which, they have been induced to apply their art to the simplest method of affording an apparatus for the filtering process, which shall not be liable to these objections. Apparatus for filtering water.

Plate VI. Fig. 1, represents the whole apparatus. Fig. 2 shews a shaded section. A is a vessel of stone-ware perforated with holes, *m*, at bottom, upon which coarse gravel, *h*, is laid, and upon that a stratum of fine gravel, and lastly fine sand, *g*. Or otherwise, the bottom may be covered with a coarse cloth, which will render the graduated fineness of the gravel and sand less necessary. Upon the top of the sand is laid a perforated and loaded board or plate of earthen ware, to prevent the sand from being disturbed when the water is poured in. B is a lower vessel, into which the filtered water from A drops, together with any sand that may escape from above. The clear water flows out through the neck *a c* into the vessel D for use.

The structure, uses, and effects of this apparatus are so obvious, that it is needless to enlarge upon them. The fineness and depth of the siliceous sand will regulate the perfection and expedition of the process; and the requisite cleanness and delicacy of the vessels and sand may be insured by changing the latter from time to time; for example, once in a fortnight or three weeks. W. N.

XVIII.

Examination of a Stone containing Potash. By FREDERIC ACCUM, *Teacher of Practical Chemistry, Pharmacy, and Mineralogy.*

To Mr. NICHOLSON.

SIR,

SINCE Klaproth has detected potash in the lepidolite, leucite, and sonorous porphyry, chemists have sought for this alkali in other minerals, and their enquiries have not been disappointed: Tromsdorff has found it lately in the augite, and no doubt this alkali will be met with in many other minerals in which it was not expected. Being called upon by the company of potters of Staffordshire to examine a variety of Introductory letter.

of ſtones employed in the manufactory of earthen-ware, a particularly exact analyſis was demanded of a ſtone, labelled *grey Corniſh* *, in which I detected this alkali. I ſhall not detain you with a circumſtantial detail of my reiterated experiments, which were undertaken with a view to learn the nature of the ſtone under conſideration, which is ſought for by the potters with avidity; I ſhall merely confine myſelf to point out its characters, as well as that examination which may ſerve to eſtabliſh the credit of my aſſertion.

I am, Sir, with reſpect,

Your moſt obedient humble ſervant,

FREDERICK ACCUM.

Old Compton Street, Soho.

Phyſical Characters of a Siliceous Stone containing Potaſh.

Phyſical characters of a ſtone containing potaſh.

THIS ſtone is found in amorphous maſſes, forming irregular ſtrata, under the ſurface of a blue clay. Its colour, when newly taken from the bed, is a greeniſh-grey interſperſed with black ſpots; but when left expoſed to the air, it acquires an aſh-grey colour. It is not very hard; its powder is white. When rubbed, it exhales a faint argillaceous odour. Its ſubſtance is coarſe or uneven, having many ſmall, ſharp, abrupt, irregular elevations and irregularities. Its fracture is very irregular. It may be eaſily ſcratched with a knife. It ſcintillates with ſteel. It is abſolutely opaque in ſmall fragments. Urged before the blow-pipe, it froths and melts into a white enamel. Treated with borax in a ſimilar way, it forms a reddiſh bead.

Its ſpecific gravity is 2,465.

ANALYSIS.

Analyſis of a ſtone containing potaſh.

Experiment I.—One hundred grains of the finely le[redacted]d ſtone, after having been previouſly ignited †, were mi[redacted]ith a ſolution of potaſh containing 400 grains of alkali, th[redacted]ture was evaporated in a ſilver capſcel to dryneſs, transferred

* I am not permitted to ſtate the exact place in Cornwall where this ſtone is found.

† 100 parts loſt, during ignition, 6 parts.

into

into a crucible of silver, and fused for half an hour. The fused mass had rather a pasty appearance, and could not be rendered perfectly fluid. Analysis of a stone containing potash.

Exp. II.—As soon as the crucible was nearly cold, it was removed from the furnace, its contents were softened by water, and the affusion of this fluid renewed from time to time, till all the fused mass was detached from the crucible: about 18 times its quantity of water were expended for that purpose.

Exp. III.—Into the obtained alkaline imperfect solution of muriatic acid was gradually poured, and the whole evaporated to dryness.

Exp. IV.—The mass was then transferred into a flask containing dilute muriatic acid, the whole was suffered to boil for a few minutes, and the insoluble part separated by the filtre. The siliceous earths thus obtained had a greyish appearance, but it acquired a white colour after having been again digested in muriatic acid, dried and ignited. It weighed 58 grains.

Exp. V.—The fluid from which this quantity of silex had been separated, together with the muriatic acid employed for purifying it, and the water expended for ablution, were concentrated to about 20 cubic inches, and then mingled, boiling hot, with a solution of carbonate of soda; the precipitate deposited was collected, and washed as usual.

Exp. VI.—As soon as the precipitate was so dry that it could be removed from the filtre without losing part of it, it was transferred into a solution of potash, and the mixture boiled for about half an hour. On suffering the alkaline solution to stand undisturbed for twenty-four hours, a powder was deposited, which, on being again boiled in a more concentrated solution of potash, remained unaltered. It was therefore collected, washed, dried, ignited, and put aside for further examination.

Exp. VII.—Into the alkaline solution, freed from this powder, I now dropt muriatic acid, till the precipitate which first appeared again vanished, and then decomposed it by the addition of carbonate of ammonia. The precipitate obtained in this experiment, after repeated ablutions and ignition, amounted to 28 grains. Experiments not essential to be stated here, convinced me that it was alumine.

 Exp.

Analysis of a stone containing potash.

Exp. VIII.—The precipitate of this experiment was dissolved in nitro-muriatic acid, and then mingled with liquid ammonia. A brown precipitate became deposited, which, after being dried, amounted to 0,25 grains. It was oxide of iron.

The fluid from which this iron had been separated, yielded no other products, on being carefully examined, except two grains of silex which escaped separation.

Thus far the stone had yielded siliceous earth, alumine and oxide of iron. I shall suppress those enquiries which proved fruitless for learning its farther composition, and state merely those which were attended with success for that purpose, and which were as follow:

Exp. IX.—One hundred grains of the stone reduced to an impalpable powder, were triturated with 600 grains of crystallized nitrate of barytes: the mixture fused till the flame of a piece of ignited wood did not become enlarged when held close over the surface of the fusing mass.

Exp. X.—The melted mass was softened with water, and digested in muriatic acid; the fluid filtered, and the residue washed.

Exp. XI.—The liquid obtained in this process I neutralized with carbonate of ammonia, and added the latter till no further cloudiness ensued. After having separated the precipitate, I evaporated the fluid to dryness: the obtained salt I transferred into a glass tube closed at one end, and committed it to sublimation.

Exp. XII.—After the sublimation had ceased (which was known by a metallic wire introduced into the tube during the process, not becoming in the least covered with a coat of salt), I broke the tube and separated the salt. This being dissolved in water and crystallized, yielded nine grains of muriate of soda.

Exp. XIII.—To decompose this salt, I redissolved it in water, and mingled the solution with nitrate of silver till no farther cloudiness appeared: I then separated the muriate of silver formed, and decomposed the nitrate of potash by heat, which yielded 4,50 of potash.

The

The analyfis being thus completed, I learnt that 100 grains of this filiceous ftone from Cornwall contain, Analyfis of a ftone containing potafh.

Silex, - - -	60
Alumina, - - -	28
Oxide of iron, - -	0,25
Potafh, - - - -	4,50
Water, - - -	6
	98,75
Lofs, -	1,25
	100

XIX.

Letter from A. CARLISLE, *Efq. on the Temperature of the Sea.*

To Mr. NICHOLSON.

DEAR SIR, *Soho Square, May* 28, 1804.

THE following table was made by Mr. R. Perrins, furgeon on the Honourable E. I. Company's eftablifhment, during a voyage to Bombay in the year 1800. The temperatures in this table, were noted at my requeft, from a defire to determine whether fifhes poffefs any other temperature than that of the water in which they live, the negative being afferted by Linnæus. As, however, this imperfect journal may affift in fimilar refearches, I beg leave to offer it for the ufe of your periodical work. Table of the temperature of the fea, &c.

I am, Sir,

Your much obliged fervant,

ANTHONY CARLISLE.

Table of the temperature of the sea, &c.

" *Register of Atmospheric Temperatures, collated with those of the Sea-Water taken on board the Honourable E. I. Company's extra Ship Skelton Castle.*"

1800.	Atmosphere.	Sea-Water.	Lat. N.		Long. W.	
Feb. 28	*	52°	42°	34′	13°	25′
March 2	*					
4	*	57	36	47	15	
7	*	62	33	19	17	34
9	*	64	31	58		50
11	*	66	30	26	20	49
13	*	67	No observ.			59
15	71°	†68	26		23	24
16	72	69½	25	49	24	
17	72	70	21	14		55
18	74	72	18	16	26	12
19	74	72	15	17		47
20	75	72	13	52	24	49
21	76	74	11	58	23	6
22	78	†76½	9	46	21	12
23	80	78	7	43	20	11
24	82	80	5	25	20	13
25	82	80	4	24	20	6
26	84	82	3	33	19	41
27	84	82	2	58	19	27
28	85	82	2	35	19	17
29	86	83	2	7	19	10
30	86	83		35	20	1
31	85	82		33 S.	19	
April 1	84	81	2	27	20	41
2	83	80	4	44	22	31
3	82	80	7	12	23	47
4	83	80	9	50	24	50
5	82	79	12	30	25	
6	80	76	15	17	25	59
7	78	76	18	13	26	5
8	78	76	20	41	26	3
9	80	78	22	22		56
10	80	78	23	28	27	20
11	80	78	24	23		37
12	78	76 Shark 88‡	24	48		32
13	76	74	26	29		22
14	74	72	27	50	24	14
15	72	70	28	42		35
16	72	70	31			45

* The atmospheric temperature was not set down during these days.

† Each trial upon the sea-water was repeated three or four times, and the same results followed.

[*Table continued.*]

Table of the temperature of the fea, &c.

1800.	Atmo-fphere.	Sea-Water.	Lat. S.	Long. W.
April 17	70°	68°	33° 10′	23° 57′
18	70	66	34 20	22 46
19	70	66	35 51	19 38
20	68	66	36 2	18 35
21	68	66	36 3	15 11
22	68	64	35 56	11 22
23	68	64		6 47
24	66	64	3[illegible] 47	3 15
25	64	62	36 34	41 Eaft
26	64	62	39	3 40
27	62	58	37 25	6 20
28	61	58	53	7 40
29	60	58	45	8 40
30	60	58	36 30	12 11
May 1	60	58	7	16 13
2	60	58	30	18 13
3	60	60	34	10 16
4	60	60	37 22*	22 25
5	60	62	10	26 38
6	58	64	36 54	50 51
7	62	66	30	34 51
8	64	66	37	39 39
9	62	64	5	43 21
10	62	64	20	45 21
11	62	64	35 43	48 14
12	64	66	33 15	49 38
13	66	66	30 35	51 18
14	68	68	27 35	52 12
15	72	70	25 2	31
16	76	74	21 21	39
17	78	76	18 52	53 16
18	78	76	16 25	54 12
19	80	78	13 27	53 29
20	78	78	12 41	50 55
21	80	78	10 56	49 41
22	80	78	7 8	49 41
23	80	80	4 9	50 32
24	81½	80	1 46	52 27
25	84	81	13 N.	54 2
26	86	82	2 32	50 59
27	84	82	4 36	56 10
28	85	82	9 26	60 51
29	85	82	9 26	60 51
30	85	82	14 31	64 41
June 1	84	82	15 56	66 59
2	86	82	17 39	69 40
3	85½	84	18 57	72
4	Arrived in Bombay Harbour.			

XX.

Observations upon the Doctrine of Count Rumford respecting the want of direct conducting Power in Fluids with regard to Heat. By CIT. BERTHOLLET.*

The doctrine of Count Rumford on the non-conducting property of fluids deserves examination.

COUNT RUMFORD has published several memoirs, by which he has endeavoured to prove that liquids and elastic fluids are not conductors of heat, and that they only transmit caloric by means of the contact with solid bodies, which is owing to the motion of their parts: as this property would make a difference between the states of a substance much greater than there is occasion to suppose in the explanation of the other phenomena; as, besides, this celebrated philosopher has fixed the attention on an object which had been neglected, and has drawn applications from it, beneficial in the arts, and in the uses of life, I think it proper to offer some doubts on the principles which he has deduced from his observations: I shall, in the first place, examine whether the facts on which he relies, cannot admit of a natural explanation from the properties which I have already analysed, or whether it will be necessary to have recourse to particular properties. But I shall attend only to the considerations which may serve to elucidate this discussion, without introducing the details it would require, if I were to examine it more fully.

Detail of Count Rumford's experiments on heated fluids.

The experiments which the author made were performed with an apparatus, of which it will be proper to insert a description. "He employed a cylindrical glass jar of 4.7 inches in diameter, and 13.8 in height; he put a known quantity of water (about two pounds) into the jar, which was intended to form a cake of ice at the bottom of the vessel. For this purpose, the jar with the water was put into a frigorific mixture of salt and ice, the action of which was not long in converting the water into a solid disk adhering to the bottom and sides of the jar; the jar was then removed, and plunged into a mixture of ice and water, to the level of the interior cake, which gave it the temperature of melting ice, or of the zero of the common thermometer. Then, after having covered the surface of the ice with a disk of paper,† hot water was poured

* From his Essai de Statique Chemique.

† Bibl. Brit.

on

on it, as gently as poſſible, to about the quantity of 74 ounces; this water was about eight inches above the ſurface of the diſk."

Detail of Count Rumford's experiments on heated fluids.

"The paper was then removed very gently, and after having ſuffered the water to remain a certain number of minutes in contact with the ice, it was poured off, and the jar with the ice which it ſtill contained immediately weighed; its difference from the primitive weight eſtabliſhed the quantity of ice which had melted while the hot water remained above it."

Having obſerved that the motion occaſioned by pouring on the hot water produced an effect which was conſiderable, and foreign to the communication of heat, the author ſucceſſively deviſed ſeveral modes of diminiſhing it. "He introduced the hot water through a wooden tube, cloſed at the bottom and pierced laterally with ſeveral ſmall holes, through which the water iſſued upon a wooden diſk, alſo pierced like a ſieve, and floating on the water as it roſe in the veſſel. This diſk was removed as ſoon as the water was poured in, and the veſſel was covered with a wooden lid, in the centre of which was ſuſpended a thermometer; finally, by previouſly covering the ice with a ſtratum of cold water, about half an inch in thickneſs, in which the perforated wooden diſk floated, the author ſucceeded in greatly diminiſhing the irregularity of the reſults."

Beſides, theſe precautions, the author ſeparated from his reſults the quantity of ice, which liquefied at the firſt inſtant, and which exceeded that which melted in the ſucceeding ſpaces of time: in theſe different experiments, while that part of the cylinder which contained the ice was kept conſtantly at the temperature of melting ice, the upper part was left in contact with the ſurrounding air, or ſurrounded with a bad conducting ſubſtance, or plunged into the mixture of water and ice: the water poured on the ice received different temperatures. I make three diviſions of the reſults of all the experiments: 1ſt. Water which was only about four degrees above zero, melted a little more ice in the ſame ſpace of time than boiling water: 2d. When the upper part of the cylinder was wrapped in a bad conducting ſubſtance, the hot water melted more ice than when it was in contact with the air: 3d. When the upper part of the cylinder was plunged into the mixture of ice and water, more ice was melted than when it was left in contact with the atmoſphere at 61° Fahrenheit's thermometer.

To

Explanation. 1. More heat will be communicated *the greater the difference of temperature.* 2. Locomotion increases this effect in fluids, and ought to be separately considered. 3. The locomotion does not follow the difference of temperature.

To explain these observations, the properties which we have recognized in liquid substances, and in elastic fluids, and from which we have inferred the changes which are effected in their different states of combinations, must be applied to the phenomena observed by Rumford.

We have seen, 1st. that the liquid particles enter so much the more rapidly into combination as they were at a greater distance from saturation, because then the force which solicits the saturation is greatest; so that the effects which depend on the communication of the temperature must be very weak, when the differences between them are but small.

2d. Locomotion, which serves to bring particles together which are at a greater distance from saturation, accelerates the effect of the mutual action by which its equilibrium is established, so that it is necessary to separate the effect which depends on this cause from that which is owing to immediate communication.

3d. Water and some other substances acquire a greater specific levity on approaching the term of congelation; whence it follows that the locomotion produced by the variations of temperature in other circumstances will be subject to modifications, which must be allowed for when water and the other liquids, which possess this property, approach the term of congelation.

To apply these properties, we must also take into consideration the direction in which the heat is communicated; for the combination of effects will be different accordingly as it is applied to the inferior or superior part of a liquid.

Locomotion is greatest *when the difference of temperature is small.*

In order that a ready motion may be established between the particles which are at the bottom of the vessel, and those at the surface, there must be but little difference between their temperature; the particles which are near the ice, and become expanded, will then raise themselves above those which have a temperature barely greater; but if the temperature should cause a great difference between the specific gravities, this motion will be much more confined, so that the ice will remain surrounded with water of its own temperature, or which is very little removed from it. It is evident, therefore, that that part of the effect which depends on the motion will be much less, when there is a great difference in the temperatures.

The effect of the heat by a fluid will be [illegible]

But when this distance in fact exists, the effect produced by the communication of the heat, independently of the motion, will

will vary according to the manner in which the temperature is preferved in the liquids. If the veffel has a non-conducting covering, the heat will be retained, and a greater quantity will be communicated than if it had been allowed to pafs into furrounding bodies.—But when the difference of the temperature of the liquid is not confiderable, as in the experiment in which water at 16° was employed, it is more advantageous to augment the effect owing to the tranflation of the particles, by cooling all the cylinder, than to preferve that which is owing to the fimple communication of caloric. It appears to me that this explanation naturally flows from the known properties of fluids, and that Rumford's obfervations do not lead us to new inductions

greateft where the difference of temperature is moft favourable to both the caufes of its communication. C. Rumford's experiments require no new law to explain them.

It muft be remarked that by feparating the effect which took place at the firft, when a confiderable difference in the temperature could occafion a quick communication, he only obferved that which was produced when there were but very flight differences between fucceffive ftrata of the liquid and the ice itfelf: now, when there is but a fmall difference of faturation, either between chemical combinations, or between the temperatures, the equilibrium is eftablifhed very flowly, and it becomes difficult to appreciate the effects.

Additional remark.

The experiments which Rumford made by plunging a fmall cylinder of iron, heated to the degree of the ebullition of water, into water and mercury ftanding over a fmall piece of ice, without producing its liquefaction, only prove that when two bodies differ but little in their temperature, the equilibrium is eftablifhed with difficulty; for it muft be obferved that the iron, which had but a little fpecific heat, and is a good conductor, muft have loft the greateft part of its heat rapidly, in that part of the liquid which it paffed gently through, and neverthelefs have raifed that of the liquid but little, or even that of the mercury, confidering the mafs of it.

Experiments with mercury and heated iron

But in thefe experiments of Rumford I find proofs of the property which he denies to liquids.

Other facts to fhew that fluids are proper conductors.

1ft. In all the experiments which I have quoted, except in thofe made with the heated cylinder of iron, the liquefaction of the ice took place in a confiderable degree, and each part liquified fuppofes a quantity of heat which would have raifed an equal weight of water from the term of congelation to 75 degrees of the centigrade thermometer.

2d. He

2d. He froze the water at the surface of mercury cooled by a frigorific mixture; the temperature of the mercury was therefore communicated to the water, and the latter yielded its caloric to the mercury, to replace that which it lost.

If the communication of heat was only the effect of the particles of a liquid, the mercury of a thermometer would scarcely change its temperature when it had arrived at the freezing point of water: in fact in several of his experiments (*Essay* 7,) Rumford supposes, that at this degree, the mercury no longer communicates heat: now a thermometer takes the temperature of neighbouring bodies very rapidly, and indicates it several degrees below the freezing point of water, and as far as its own congelation; then it conducts itself like the solid bodies, and its dilatations become proportionably smaller than the preceding.

Mercury conducts better than water, but being denser, has less locomotion, ergo, &c.

Rumford has proved that the conducting power of mercury is to that of water as 1000 to 313.

This effect of the mercury, which takes the temperature of the system in which it is placed more rapidly than the water, although it has a much greater specific gravity, and is much less dilatable by the same degrees of heat, and consequently the heat will cause much less locomotion in its particles than in those of water; this effect I say, proves that the changes of temperature do not depend on the immediate communication and the changes of specific gravity which produces the approximation of the particles of unequal temperatures, but also on the better or worse conducting property of each substance.

Rumford neglects the radiant heat.

3d. Rumford paid no attention to the radiant caloric, nor did he make any allowance for it; nevertheless the communication of heat established by its means between solid bodies and liquids, through the gases, cannot be doubted, and it may be remarked that when he brought a heated bullet near to ice and tallow, a communication of heat took place which melted the surface of both, without it being possible to attribute this communication to a circulation such as he thinks is necessary.

Experiments of other philosophers.

The ingenious experiments of Rumford have employed the talents of several philosophers, who have already proved that the principles to which they led were not conformable to the true results of observation.

Nicholson found heat to pass downwards through a fluid.

Nicholson, in conjunction with Pictet, made some experiments by which he proved, that, on heating a liquid at the surface, by the superposition of a body, the heat penetrated, and

and raifed a thermometer placed at the bottom of the liquid: to avoid communication by the fides of the veffel, a bad conducting fubftance was made choice of, and he afcertained, by means of a thermometer placed in the fame liquid near the fide of the veffel, that no current was eftablifhed which differed in the temperature: finally, the motion of the bubbles which were difengaged, and the other appearances of the liquid, convinced him that currents were not formed.

In thefe experiments * it was proved, that liquids were different in their conducting faculty; the penetration of the heat from the top to the bottom, was five times flower in oil than in mercury.

—and that oil conducted much worfe than mercury.

Rumford fuppofed that the flighteft changes of fpecific gravity were accompanied by a locomotion, which produced a current, and he endeavoured to render it vifible, by expofing an alkaline liquor, in which were fufpended very fmall fragments of amber, which he found had the fame fpecific gravity as the liquid, to a change of temperature: but Thomfon has fhown †, that the motions obferved in thefe moleculæ were illufory, and that, in thefe variations of temperature, which are gradual, they appear to be owing only to the difference of fpecific gravity which they acquire, and to the adherence of air-bubbles, fo that fome of thefe moleculæ move in contrary directions, and run againft each other without following the direction of the currents, he has alfo fhown that thefe floating corpufcules might receive different motions while the ftrata of the liquid maintained a perfect tranquillity: he put water, tinged blue by juice of red cabbage into a glafs veffel; he afterwards poured clear water on it with great precaution, by means of a tube with a capillary extremity; thus he kept the two liquids feparate and diftinct; he then heated the veffel gently at the bottom: it is manifeft that if a current had been eftablifhed, it would have been marked by the coloured liquid, but the feparation of the two liquids was preferved unconfufed; moreover the corpufcules put into the firft liquid moved upwards and downwards, and croffed the line of fepa-

Thomfon found that the motions of folids do not prove any currents in the fluids,

—the floating folids rofe and fell thro' two quiefcent fluids of different colours.

* Bibl. Brit. Tom. XVIII. or Philofophic Journal, Quarto feries. V. 197.

† Nicholfon's Journal, Octavo, for Feb. 1802. See alfo a memoir by this philofopher, in the Journal for March, 1801, containing the earlieft experimental examination of the Count's doctrine.

ration

ration without producing the mixture of the two fluids, fo that their various motions were not the effect of a current which carried them with it, and neverthelefs, the heat was communicated to all the liquid. The propagation of the heat, and the agitation of corpufcules, which have nearly the fame fpecific gravity, may therefore take place, independently of the circulatory motion, which is only eftablifhed when there is a difference of temperature of a certain intenfity between the different ftrata of a fluid.

Murray made experiments in a veffel of ice, containing a fluid. The heat paffed downwards,

Murray has oppofed Rumford's opinion with experiments ftill more direct, and not lefs conclufive; * he placed the bulb of a thermometer in a cylinder of ice, which he filled alternately with oil and mercury; he afterwards brought a heated body near the furface of the liquid; the thermometer rofe feveral degrees in both experiments; but the heat could not have been conveyed by the fides of the ice whofe furface would have abforbed and liquefied; no current was eftablifhed, for the moleculæ of the liquid having become lighter, could not take a contrary direction, and the author avoided ufing water, which contracts on paffing from the degree of congelation to a temperature a little more raifed: the heat muft therefore have been communicated to the bulb of the thermometer without the eftablifhment of fuch a current as is fuppofed to be requifite; and that which ferved to dilate it was only the excefs of what had liquefied part of the ice.

—and mercury conducted better than oil.

The obfervations of Murray prove at the fame time that mercury is a much more effective conductor of heat than oil, for the elevation of the thermometer was manifefted by its intermedium in a much fhorter time, and more ice was liquefied.

* Ann. de Chim. Floreal, An. X. or Philof. Journal, Octavo, I. 165. 241. The experiments of Thomfon and Murray, originally appeared in our Journal, and thofe of Count Rumford are alfo given, for which fee the Indexes.

(To be continued.)

On

XXI.

On the Difficulty of obtaining Alumine in a State of Purity. *By R. T.*

To Mr. NICHOLSON.

SIR,

YOUR Journal being open to every difquifition which may contribute to the progrefs of fcience, it may not be deemed impertinent to afk you, or fome of your correfpondents, to point out the beft method of preparing pure alumine.

You may, probably, refer me to our modern authors on chemiftry, but I aver that the methods therein recommended do not anfwer the purpofe. For if a faturated folution of alum of commerce, be decompofed by a like faturated folution of a carbonated alkali, the alumine obtained is harfh to the touch, rather fpungy, and ftrongly adheres to the tongue.

Alumine obtained by mixing faturated folutions of alum and alkali,

This earth, although wafhed as often as you pleafe, always reddens the blue juice of the flowers of mallow, as well as of other delicate vegetable blues. It may be wholly diffolved in about 100 parts of boiling water: and the folution becomes very turbid by muriate of barytes.

appears to contain acid, and is foluble.

If, on the other hand, a dilute folution of alum be decompofed by another of alkali, a quite different product will be obtained. The alumine produced, on being deficcated is not porous, but fplits into pieces like ftarch; and has, before it is nearly dry, a certain degree of tranfparency; it breaks with a fmooth and nearly conchoidal fracture; it does not adhere to the tongue like the former, and has no earthy appearance. This, like the former, cannot be freed from the adhering acid. It alfo changes fine vegetable blues to red, although ever fo much wafhed. What is the reafon of this? Is there no method of forcing this earth from the adhering acid? or is it perhaps a characteriftic of the earth itfelf, to redden vegetable blues. If you will pleafe to anfwer this queftion, or point out a better method for procuring this earth pure, you will much oblige,

Dilute folutions afford alumine of another appearance,

likewife acid.

Camden-Town, SIR, Your conftant reader,
May 28, 1804. R. T.

REPLY.

THIS letter having arrived fo late in the month, I can only for the prefent offer it to my other correfpondents.

SCIENTIFIC

SCIENTIFIC NEWS.

New Earth.

New earth.

A NEW earth has been discovered by Professor Klaproth of Berlin, in an ore which was hitherto supposed to contain tungsten, to which he has given the name of *ochroit* earth, the mineral which contains it he has called ochroït, (ochroïtes.) This earth seems to form the connecting link between the earths and the metallic oxides. Like yttria, it produces a reddish coloured salt with sulphuric acid; and is precipitable by all the prussiates; but it is distinguished from yttria by not forming sweet salts, and by not being (at least much less) soluble in carbonate of ammonia, and by acquiring when ignited, a light brown colour. This earth farther differs from yttria, by not being soluble either by borax nor by phosphates, with which yttria fuses into a colourless pellucid bead. Its other characteristics, and method of obtaining will be given in our next Journal.

Suberic Acid from Paper *.

BRUGNATELLI has observed, that when nitric acid is made to act upon paper, a large quantity of suberic acid, mixt with oxalic acid, is obtained. This proves that Fourcroy was right, in placing cork among the immediate principles of vegetables.

Easy and expeditious Method of preparing Copal Varnish †.

DEMMENIE, an ingenious glass blower, has noticed that the solution of copal may easily be effected, by exposing it to the vapours of alcohol or oil of turpentine. For that purpose an alembic may be filled $\frac{1}{4}$ with either of these fluids, and some pieces of copal suffered to be suspended by threads in it, over the surface of the fluid. After having made the alcohol or oil of turpentine to boil, the copal becomes liquefied, drops into the fluid and becomes dissolved. When no farther

* Gehlen's *Journal of Chemistry*: Vol. I. p. 3. page 340.
† Ibidem.

solution

solution takes place, the whole is suffered to cool, and the solution of copal is decanted from the undissolved part. The varnish has no more colour than the copal itself.

Large Piece of Amber *.

A PIECE of amber weighing 13lb. 7 oz. 9 scr. and measuring 318¾ cubic inches, has lately been found at Schlapacken, near Gumbinnen and Insterburg in Germany, which is the largest mass of amber hitherto found. Its colour is a pale yellow, intersected with several lines. Its value is estimated at about 40,000 dollars. Large specimen of amber.

Fluoric Ether †.

15 oz. Previously ignited and pulverized fluate of lime, were introduced into a retort containing 10 oz. of highly rectified alcohol, and an equal quantity of sulphuric acid of 1,860 spec. grav. and the mixture distilled to dryness. During the distillation, a large quantity of gas was evolved, which burnt with a beautiful blue flame, and diffused an odour resembling phosphorated hydrogen. During the combustion of this gas, vapours of fluoric acid were precipitated. The product which had been obtained during the distillation was again distilled to one half, and the product which passed over, was poured into a vial containing water. No heat was produced, nor did the two fluids mix. It was therefore ether. But as its taste was sour, I added to it a solution of potash; this instantly separated a considerable quantity of silex, and the whole became converted into a gelatinous mass. The whole mass, on being again distilled, yielded pure fluoric ether. It greatly resembled sulphuric ether; its specific gravity was 0,720; it burnt with a blue flame; its taste was bitter, and greatly resembling bitter almonds.

New Method of preparing Nitric Ether. By BRUGNATELLI ‡. Nitric ether.

INTRODUCE into a tubulated retort, one ounce of sugar, and pour over it two ounces of highly concentrated alcohol. Adapt to the retort a capacious receiver surrounded with

* Ibid. † Ibid. ‡ Ibid.

cloth

cloth dipt into water; and fecure the junctures of the veffels by furrounding them with flips of paper only. Having done this pour through the tubulure of the retort 3oz. of concentrated nitrous acid; a violent action takes place, the fugar becomes diffolved, and the alcohol converted into ether, paffes over into the receiver; its quantity is nearly equal to the alcohol employed.

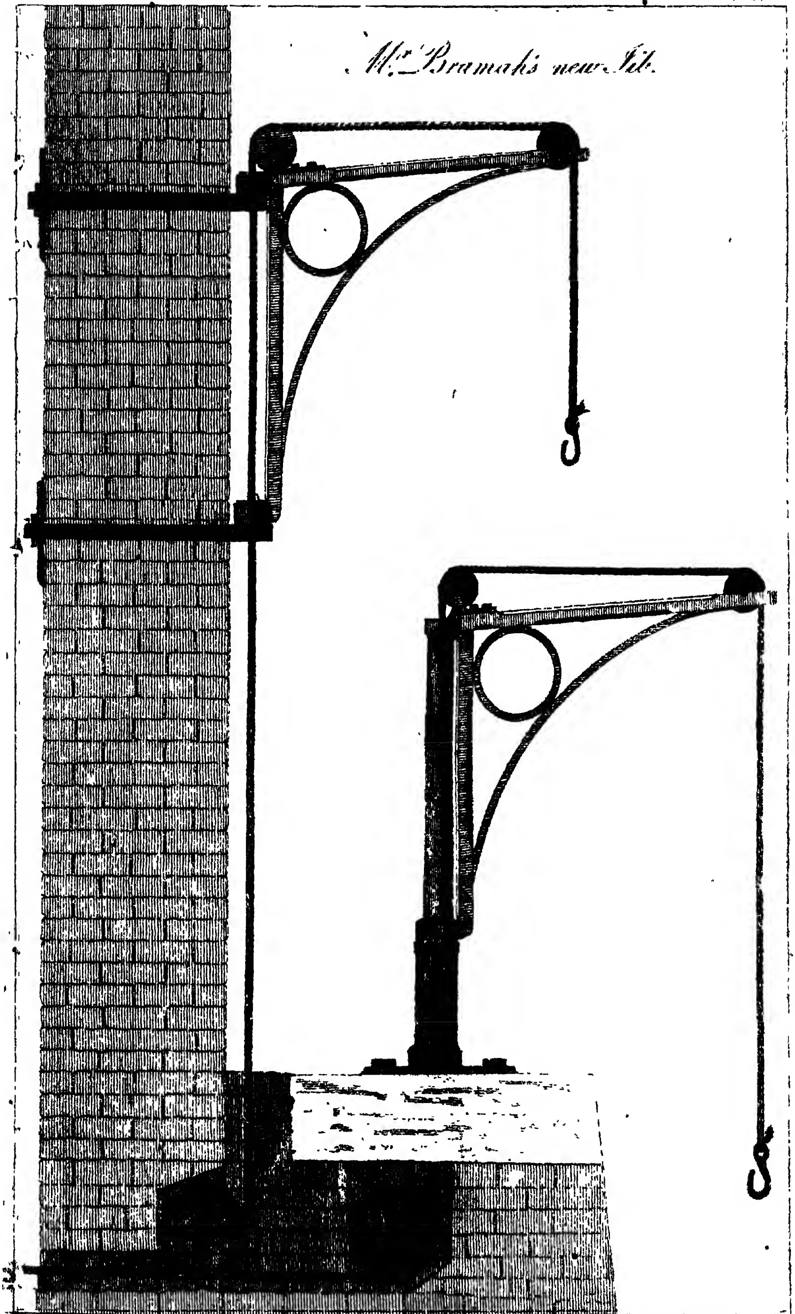
Accenfion of Sulphuretted Hidrogen Gas, by the Affufion of Nitrous Acid. By Profeffor LICHTENBERG *.

Afcenfion of fulphurated hydrogen gas.

ATTEMPTING to illuftrate the decompofition of fulphuretted hydrogen gas, I filled a bottle with it, capable of holding about 18oz. of water. Having done this, I poured into it at once, $\frac{3}{4}$ of an ounce of nitrous acid; a hiffing noife took place, and much red vapour was difengaged, which in order not to moleft my auditors, I confined in the bottle, by corking it. No fooner had this been accomplifhed, the mixture exploded with a loud report, accompanied with a blue flame †. The pieces of glafs were thrown to a confiderable diftance, the larger ones were covered with fulphur.

* Ibid.

† For the fuccefs of this experiment, it feems to me to be neceffary that the gas muft be obtained by decompofing water by means of fulphuret of iron and an acid; for it always failed in my hands, if an earthy or alkaline fulphuret had been made ufe of for the production of it F. A.


M.r Bramah's new Jib.

Filtering Apparatus.

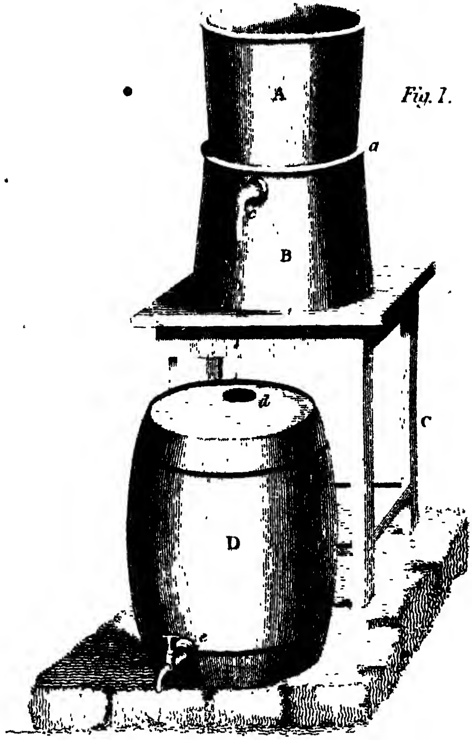


Fig. 1.

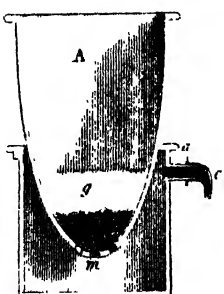


Fig. 2

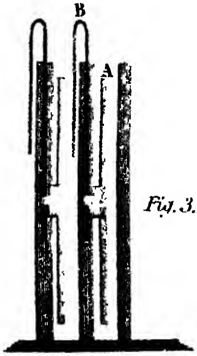


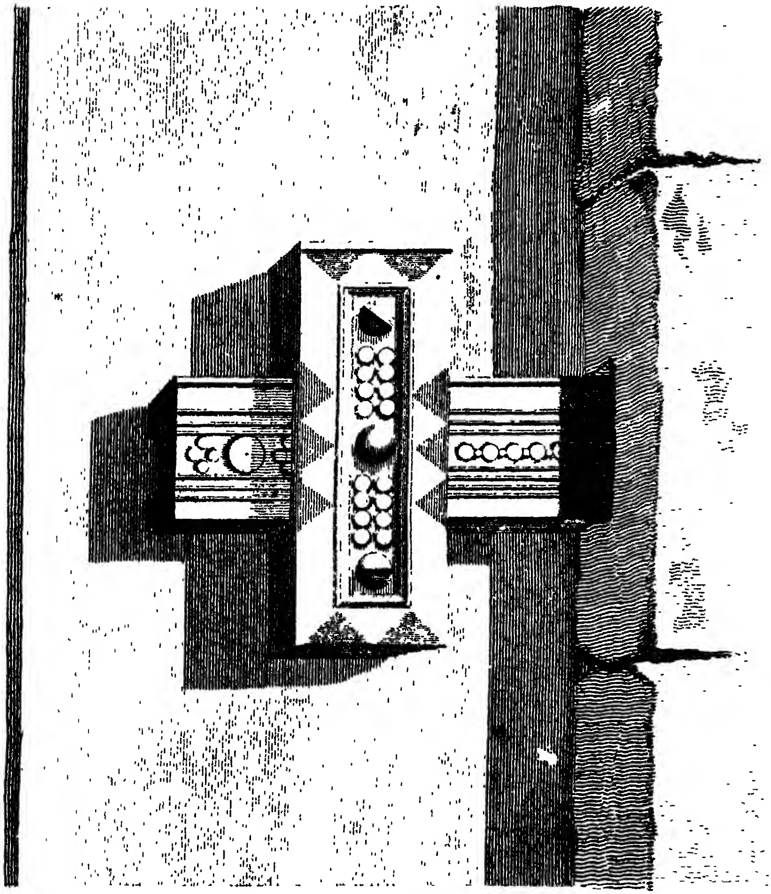
Fig. 3.

Galvanic Apparatus.

Drawn by Blunt

Engraved by Mutlow

Ancient wooden Lock,
used in Egypt, and in most parts
of the Turkish Empire.



Ancient Egyptian Lock, developed.

Fig. 1

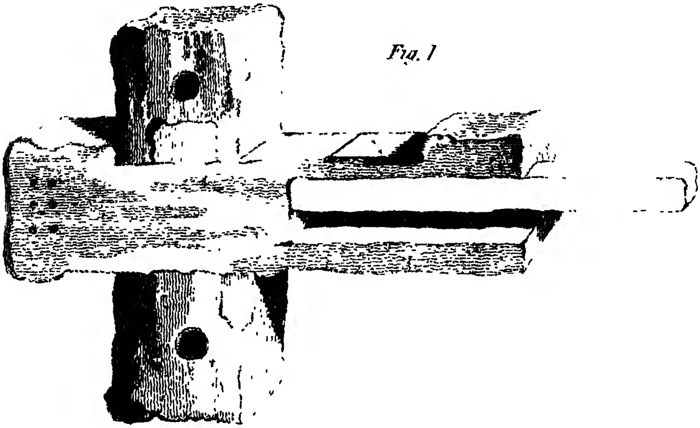


Fig. 2.

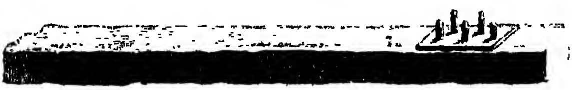
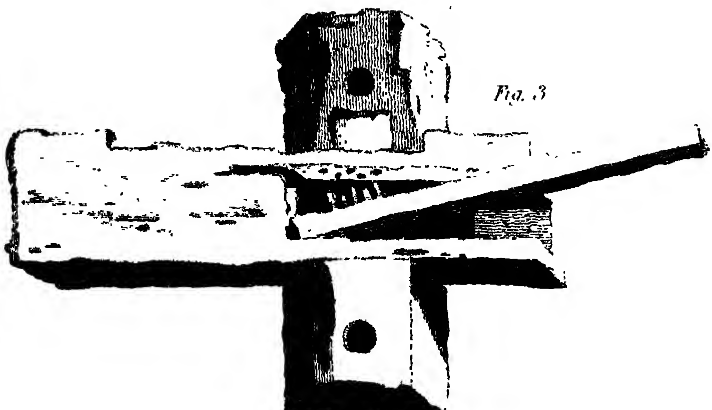


Fig. 3



A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1804.

ARTICLE I.

On the supposed Chemical Affinity of the Elements of Common Air; with Remarks on Dr. Thomson's Observations of that Subject. In a Letter from Mr. J. DALTON.

To Mr. NICHOLSON.

SIR,

IN a former letter, inserted in your Journal (new series, vol. III. page 267) I endeavoured to shew the absurdity of the notion of atmospherical air being a chemical compound of azotic and oxigenous gases. Besides the difficulty, or rather impossibility on the one hand of conceiving how two elementary particles, constantly repelling each other, should notwithstanding be held together by a principle of cohesion or chemical affinity; or on the other hand, supposing the two atoms to combine, and form one centre of repulsion, how atmospheric air should differ from nitrous gas, &c. There are a variety of facts which oppose the doctrine so forcibly that I have for some time wondered on what grounds those who are still its adherents defended it. Dr. Thomson, in the second edition of his chemistry, vol. III. page 316, after reviewing the opinions of different philosophers on this head, and amongst others my own, concludes that air is a chemical compound; he assigns the four following reasons for the conclusion, which, from his extensive ac-

Difficulties respecting the position that atmospheric air is a chemical compound.

Dr. Thomson's arguments in favour of that doctrine.

acquaintance with authorities, may fairly, it is presumed, be deemed the most cogent that have been offered on that side of the question. It is the object of this communication to shew their insufficiency.

1. The constant proportion of oxigen and azot supposed to be regulated by affinity, or increased attraction of the abundant gas,

1. The constant proportion of azot and oxigen in the atmosphere is considered as an argument for their being held by affinity. So indeed it may; but it is equally in favour of my hypothesis, and therefore nothing tending to decide the question can be obtained from it. For, let part of the oxigen be abstracted any where from the atmosphere; then the azot may be supposed to attract the oxigen from the vicinity, and thus the equilibrium be restored: but it is certainly equally satisfactory to suppose that the oxigen in the vicinity, meeting with a less repulsive power from the deficient quarter, nothing prevents its diffusion into that quarter but the azot previously there, which, by hypothesis, can only *retard*, but by no means *prevent* the effect. Thus then, whether the azot *attract* the oxigen, or the oxigen *repel* itself, the effect is precisely the same. From this fact simply, it is impossible therefore to decide the merits of either theory; but if it be found that any one gas diffuses itself in any other, with nearly the same celerity, it will be a presumption in favour of my hypothesis; if otherwise, it may be urged that the quicker diffusion is owing to the stronger affinity. I have made a great number of experiments on this head, but could not find any remarkable difference in the time and circumstances of diffusion of the same gas.

—but this may with more probability be ascribed to *diminished repulsion* of the deficient gas: For, &c.

2. Humboldt and Morozzo's experiments; that a mere mixture of the gases differs from atm. air:—not credited.

2. It is said the experiments of Morozzo and Humboldt shew that air possesses different properties from a mere mixture of its two component parts. I do not credit the experiments.—Humboldt finds a *variable* quantity of oxigen, from 25 to 30, or more per cent. in the air; whereas others who are more accurate, find but 21, or at most 22, and that *constant*. It is no wonder then, if he mix 28 oxigen and 72 azot, that the mixture diminishes nitrous gas more than air, and supports combustion and animal life for a longer time.

3. Different combustibles absorb different quantities of oxigen from common air,

3. Different combustibles are capable of absorbing different portions of oxigen from a given quantity of air. Phosphorus 22 per cent. Sulphur, 8, &c.—The only inferences I draw from these facts are, that phosphorus will burn in oxigen of any density, that sulphur will not burn in oxigen unless it be of $\frac{1}{7}$ of atmospheric density or more. The difference in the pheno-

mena of combustion in air, and in oxigen is not to be ascribed to the combination of azot and oxigen, but to the less density of the latter, ($\frac{1}{5}$) of what a pure atmosphere of the same gas would be. From an incidental but imperfect trial I made, in conjunction with Mr. Davy last winter, I have no doubt but iron wire would burn in common air of five times the density with brilliancy as in an atmosphere of pure oxigen of common density. At any rate it is notorious that as the density of common air is increased, combustion in it becomes more vigorous. Though I have never attempted combustion in an atmosphere of pure oxigen of $\frac{1}{5}$ the common density, I can scarcely doubt that the appearancees would be much the same as in the open air. It is probable therefore that the facts under this head, if duly investigated, would turn out in favour of the hypothesis of air being a mixture.

Ascribed merely to the various densities of oxigen in which they cease to burn.

Proposed experiments to decide this point.

4. "A gas no way distinguishable from common air frequently makes its appearance during the preparation of nitric acid; and Mr. Davy decomposed nitrous oxide, by passing it through a red hot tube, and converted it into nitric acid and a gas, which possessed the properties of common air; now if air were a mere mixture, it is infinitely improbable that its two constituent parts should be evolved during such processes exactly in the proportion that exists in common air."—Granted; but as the force of this argument rests upon the *exact proportion* of oxigen and azot in the gases so evolved, that is, upon their being constituted always of 21 per cent. oxigen, and 79 azot, the facts should be made out accordingly. Dr. Priestley is the only one I know of, who has particularly examined the gas produced in the preparation of nitric acid, and he found it to have *much more* oxigen than common air. Mr. Davy in his analysis of nitrous oxide, found the gas analogous to atmospheric air always to contain *less* oxigen, though it was nearly of the atmospheric standard.

4. Gas resembling common air, obtained in preparing nitrous acid:

Answer. The precise resemblance, or proportion of parts, has not been shewn.

The quick ascent of hidrogen and the descent of carbonic acid, have been objected to my hypothesis as facts that prove the operation of the laws of specific gravities on elastic fluids. No doubt can exist that a portion of elastic fluid completely insulated, as a balloon, or a bubble of carbonic acid or hidrogen, surrounded by a film of water, is subject to the laws of gravitation, and rises or falls in elastic fluids on the same prin-

The statical ascent or descent of the gases affords no proof against the law of their general diffusion.

 ciple

ciple as it rises in water; the same must be allowed when a vessel, containing a considerable portion of elastic fluid, is suddenly exposed at some surface to the atmosphere; in this case, the fluids *must operate upon each other* for a few moments in a collected capacity, as in elastic bodies; because the diffusive or repulsive force by which they constantly tend to dispersion, is comparatively slow in producing the ultimate effect, being in this respect exactly similar to chemical affinity, the operation gradually diminishing as the effect draws towards a conclusion.

For the separate masses are carried by their relative gravity more speedily than the diffusion can take place.

Nothing more therefore can be inferred from the facts abovementioned, than that gravity overpowers, and for a moment obliterates the effect of that cause which in other cases slowly produces the dispersion of the fluid, whether it be attraction, as commonly supposed, or repulsion, as I suppose. Chemical philosophers have not enquired sufficiently into the effects of exposing gases in different circumstances to the atmosphere; all that we are usually told is, that a jar filled with hidrogen and uncovered, loses its gas in a few seconds; but if inverted, it remains nearly pure for a considerable time, &c. I find that a cylindric jar of 7 inches depth and 2½ diameter, being filled with hidrogen, and inverted, loses more than half of its gas in two minutes, and there is so little left as scarcely to explode in five minutes.

But the diffusion is much speedier than is generally taught.

If a tube, 12 inches long and ½ inch diameter, be filled with hidrogen, and exposed in like manner to the atmosphere, it will lose half its gas in five minutes, and that the same, whether it be held up or down or horizontal. Here we see effects that cannot be accounted for by gravity, that are produced in opposition to its agency, and where indeed it is almost obliterated by the action of some more powerful cause. Let the advocates for the atmosphere being a chemical compound attend to such facts as these, and they will soon find themselves reduced to acknowledge that *all gases have the same affinity for one another*, a position which their doctrine ultimately tends to establish.

The facts, if ascribed to affinity, would shew that it is the same between all the gases—and even between a gas and a vacuum.

Indeed it is the same with regard to air and vapour, of water, ether, or of any other fluid; that is, all kinds of gas or mixtures of gases, have the same affinity for the same vapour, and even a torricellian vacuum possesses just the same affinity as any of them, judging from the quantity evaporated, and force of the vapour in a given volume. If any one doubt it, he may easily satisfy himself by throwing up a drop

a drop or two of ether into the vacuum of a common barometer; if the temperature be 68°, the mercury will fall 15 inches nearly; at the same time, if ether be admitted to a given bulk of any kind of gas, subject to the pressure of the atmosphere, the volume will be doubled, clearly shewing that the elastic vapour from the ether is the same in both cases, namely, an independent fluid of 15 inches force.

I cannot dismiss this subject without observing, in justice to Dr. Thomson, that he has entered more clearly into my views of these subjects than any other of our own country who has animadverted upon them. There are certain principles, however, which he, with most chemists of the present day, embraces, which are, according to my experience, decidedly erroneous. One of these is, *that water dissolves air*. An excellent paper of Mr. W. Henry, on the absorption of gases by water, in the Philos. Transactions for 1803, has shewn us sufficiently in what light we should view the supposed solution of air in water. Certainly air that is retained in water by mechanical force, and which always escapes when that force is withdrawn, cannot with any propriety be said to be held by chemical affinity.

Water does not dissolve air.

Dr. Thomson has been misinformed respecting my opinions on the expansion of liquids. In vol. i. page 343, he gives it as my suggestion, that all liquids expand the same quantity from their freezing to their boiling temperatures. I never entertained such an opinion; and it is certainly erroneous. My idea is, that pure and homogeneous liquids, such as water and mercury, expand according to the square of the temperatures from the points at which they congeal; but I have not yet found a law to regulate the *relative* expansions of these and other liquids.

The author does not affirm that the expansion of all liquids by heat is the same between congelation and boiling; but that any single fluid expands according to the square of the temperature from its freezing point.

I am your's, &c.

J. DALTON.

Manchester,
June 16, 1804.

Easy

II.

Easy Methods of completing the Tables of Squares and Cubes. In a Letter from H. G.

To Mr. NICHOLSON.

SIR,

Computation of squares:

IN the last number of your Journal, Mr. Councer's extensive table of squares is stated to be deficient from No. 28261 to 29061. This deficiency may very easily be filled up by this rule. To the square of any given root add twice that root + 1; the product will be the square of the next root.

Of cubes; half the series;

The table of one half the cubes in Mr. Councer's table, may easily be made, examined, and added to, by the following rule: Multiply the cube of any given root by 8; the product will be the cube of twice the next root.

the other half.

The other half of the cubes in the same table, and also all those already found as above, may easily be found from any two cubes and their roots in succession being given. Thus: From the given cube of the largest of the given roots, subtract the given cube of the next less given root; to the remainder add six times the largest given root, and also the given cube of the largest given root; the sum will be the cube required; as will appear from the following example:

	Roots.	Cubes.	
Give	9999 = A =	999700029999	Required the cube
	D = 10000 = B =	1000000000000	of the root 10001.
	B — A = C =	299970001	Remainder.
	6 D =	60000	Six times the largest given root.
	B + C + 6 D =	1000300030001 =	Cube required of root 10001.

I am, Sir,

Your most humble servant,

East Smithfield. H. G.

Problem

III.

Problems in Spheroidal Triangles. By PEREGRINUS PROTEUS.

To Mr. NICHOLSON.

SIR,

THE solution of the problem relating to the figure of the earth, in my first letter *, led to some new properties of spheroidal triangles, from which I shall endeavour, in this, to deduce a few rules, that may be applied with success in trigonometrical surveys. It will be found that, though the general formulæ be complex, yet, in the cases that occur in practice, they admit of being sufficiently simplified; for not only may the terms involving the second and higher powers of the compression, but also frequently the differences between the longitudes and latitudes of the stations, be rejected. New problems in spheroidal triangles.

1. 'Having given the latitudes of two places on the surface of the earth, and the length of the straight line or chord joining them, it is required to find their difference of longitude?' Prob. I. Given two latitudes and chord of arc: Required diff. longit.

Let λ, ϕ be the latitudes of the two places, D their distance in fathoms, a the radius of the equator in fathoms, $\frac{c}{a} = \delta$ the compression at the poles, d such that sin $\frac{1}{2}d = \frac{D}{2a}$, and ω' the difference of longitudes of two places, whose latitudes are λ, ϕ, and distance d on the sphere. Then by the formula, page 16,* the true difference of longitude ω is equal to $\omega' +$ $Q \times \frac{c}{a} = \omega' + Q\,\delta$, if we reject the higher powers of δ, in which †

$$Q = \frac{2\,(\sin\lambda - \sin\phi)^2 - 2\sin\frac{1}{2}d^2\,(\sin\lambda^2 + \sin\phi^2)}{\cos.\lambda\,\cos.\phi\,\sin\omega'}.$$

Now because in trigonometrical surveys, d, ω', and $(\lambda - \phi)$ are small, we may take $\frac{2\,(\sin\lambda - \sin\phi)^2}{\cos.\lambda\,\cos.\phi\,\sin\omega'} =$

* Journal for May.

† There are some typographical and other errors in the paper referred to, which I have taken notice of at the end of this letter.

8 cos.

$$\frac{8 \text{ cof.} \left(\frac{\lambda+\phi}{2}\right)^2 \text{ fin} \left(\frac{\lambda-8}{2}\right)^2}{\text{cof. } \lambda \text{ cof. } \phi \text{ fin } \omega'} = 4 \text{ fin} \frac{\lambda-\phi}{2} \times \frac{\lambda-\phi}{\omega'}, \text{ and}$$

$$\frac{2 \text{ fin} \frac{1}{2} d^2 (\text{fin } \lambda^2 + \text{fin } \phi^2)}{\text{cof. } \lambda \text{ cof. } \phi \text{ fin } \omega'} = 4 \text{ tang. } \lambda \text{ tang. } \phi \times \frac{\text{fin} \frac{1}{2} d^2}{\text{fin } \omega'} =$$

$2 \text{ tang. } \lambda \text{ tang. } \phi \text{ fin } \frac{1}{2} d \times \frac{d}{\omega'}$; wherefore Q is equal to 4 fin

$\frac{\lambda-\phi}{2} \times \frac{\lambda-\phi}{\omega'} - 2 \text{ tang. } \lambda \text{ tang. } \phi \text{ fin } \frac{1}{2} d \times \frac{d}{\omega'}$, and

$\omega = \omega' - \delta. \left\{ \text{tang. } \lambda \text{ tang. } \phi \times \frac{d^2}{\omega'} - \frac{2(\lambda-\phi)^2}{\omega'} \right\}$ nearly; where the correction $Q\delta$ will be given in feconds of a degree, if d, ω', and $(\lambda-\phi)$, be expreffed in feconds.

By fpherics cof. $\omega' = \frac{\text{cof. } d - \text{fin } \lambda \text{ fin } \phi}{\text{cof. } \lambda \text{ cof. } \phi}$, whence we find, when d and ω' are fmall, $\omega' = \sqrt{\left\{ \frac{(d+\lambda-\phi)(d-\lambda+\phi)}{\text{cof. } \lambda \text{ cof. } \phi} \right\}}$ without any fenfible error, which value fubftituted in the formula $\omega = \omega' + Q\delta$, will give the true difference of longitude required.

When the meafured chord is perpendicular to the meridian, ϕ is nearly equal to λ, and confequently $\omega' = \frac{d}{\text{cof. } \lambda} = \frac{D}{a \text{ cof. } \lambda}$, and $\omega = \frac{D}{a \text{ cof. } \lambda} \times (1 - \delta \text{ fin } \lambda^2)$, or $\omega = \frac{D}{\text{cof. } \lambda \, (a + c \text{ fin } \lambda^2)}$; but $a + c \text{ fin } \lambda^2$ is equal to the radius of curvature of the perpendicular to the meridian, in the latitude $\lambda = AM$ in the figure (*Journal for May*), which put $= R$, and there refults $\omega = \frac{D}{R \text{ cof. } \lambda}$, which correfponds exactly with one of Legendre's theorems, (*Mem. Acad.* 1787).

Example. Let $\lambda = 50° \; 44' \; 23'',71$, the latitude of Beachy Head, $\phi = 50° \; 37' \; 7'',31$, the latitude of Dunnofe, $D = 56566,57$ fathoms, and $d = 3496740$. Then becaufe fin $\frac{1}{2} d = \frac{D}{2a}$, d is $= \frac{D}{a}$ nearly $= 3336'',73$, $\omega' = 1° \; 27' \; 0'',65$, and $\omega = \omega' - 3105'',74 \, \delta$; in which, if we fuppofe $\delta = \frac{1}{252}$, we fhall have $\omega = 1° \; 26' \; 47'',93$.

2. 'Having

2. 'Having given the latitude of a place, and its longitude and distance from another place, it is required to find the other latitude?'

Prob. II. Given one lat. and long. and distance from another place. Req. lat. of this last.

Let λ be the given and ϕ the required latitude; D the measured distance, $d=\frac{D}{a}$, ω the difference of longitude, and ϕ' the latitude on the sphere found from λ, ω, d. Then by spherics cos. λ cos. ϕ' cos. ω+sin λ sin ϕ'=cos. d, whence, when d, ω, and $\lambda-\phi'$ are small, there results $\lambda-\phi'=\pm\sqrt{(d^2-\text{cos. }\lambda\ \text{cos. }\phi'.\ \omega^2)}=\pm\sqrt{(d^2-\omega^2\ \text{cos }\lambda^2)}$ nearly. But by the theorem, page 16, if $\phi=\phi'+x$, we find $x=Q\ \delta$, in which the variable quantity Q is $=\frac{2\,(\text{sin }\lambda-\text{sin }\phi')^2-\frac{1}{2}d^2\ (\text{sin }\lambda^2+\text{sin }\phi'^2)}{\text{cos. }\lambda\ \text{sin }\phi'\ \text{cos. }\omega'-\text{sin }\lambda\ \text{cos. }\phi'}=$

$$\frac{\frac{1}{2}d^2\,(\text{sin }\lambda^2+\text{sin }\phi'^2)-2\,\text{cos.}\left(\frac{\lambda+\phi'}{2}\right)^2.\,(\lambda-\phi')^2}{2\ \text{cos. }\lambda\ \text{sin }\phi'\ \text{sin }\tfrac{1}{2}\omega'^2+\text{sin }(\lambda-\phi')};\ \text{or}$$

$$Q=\frac{\frac{1}{2}d^2\ (\text{sin }\lambda^2+\text{sin }\phi'^2)-2\ \text{cos.}\left(\frac{\lambda+\phi}{2}\right)^2.\,(\lambda-\phi')^2}{\text{cos. }\lambda\ \text{sin }\phi'\,\text{sin }\tfrac{1}{2}\omega'\times\omega'+(\lambda-\phi')}.$$

Now when λ and ϕ are nearly equal, this formula becomes,

$$Q=\frac{\text{sin }\lambda^2.\ d^2-2\ \text{cos. }\lambda^2\ (\lambda-\phi')^2}{\omega.\ \text{cos. }\lambda\ \text{sin }\phi'\ \text{sin }\tfrac{1}{2}\omega+(\lambda-\phi')}=$$

$$\frac{\text{sin }\lambda^2\ d^2-2\ \text{cos. }\lambda^2\ (\lambda-\phi')^2}{\text{cos. }\lambda\ \text{sin }\lambda\ \text{sin }\tfrac{1}{2}\omega.\ \omega+(\lambda-\phi')}.$$

3. 'Having given the latitude λ, the horizontal angle α, and the distance D, it is required to find the difference of longitude?'

Prob. III. Given one lat. with the horiz. angle and distance: To find diff. long.

Let ϕ be the latitude, and ω the longitude of the place required; with the latitudes λ, ϕ, and distance $d=\frac{D}{a}$, on the sphere, to find the difference of longitude ω', and the horizontal angle α' at λ.

Then will sin $\omega'=\frac{\text{sin }d\ \text{sin }\alpha'}{\text{cos. }\phi}$, or $\omega=\frac{\text{sin }\alpha'.\ d}{\text{cos. }\phi}$; but by first question $\omega=\omega'-\delta\left\{\text{tang. }\lambda\ \text{tang. }\phi'\times\frac{d^2}{\omega'}-\frac{2\,(\lambda-\phi)^2}{\omega'}\right\}$;

wherefore $\omega=\frac{d\ \text{sin }\alpha'}{\text{cos. }\phi}.\left\{1-\delta\left(\frac{\text{tang. }\lambda\ \text{sin }\phi\ \text{cos. }\phi}{\text{sin }\alpha'^2}-\frac{2\ \text{cos. }\phi^2\ (\lambda-\phi)^2}{d^2\ \text{sin }\alpha'^2}\right)\right\}=\frac{d\ \text{sin }\alpha'}{\text{cos. }\phi}\left\{1-\delta\left(\frac{\text{sin }\lambda^2}{\text{sin }\alpha'^2}-\frac{2\ \text{cos. }\lambda^2\ (\lambda-\phi)^2}{d^2\ \text{sin }\alpha'^2}\right)\right\}$.

Now

Now by spherics $\left(\frac{\lambda-\phi'}{\mu}\right)^2$ is $=$ cof. α'^2 feré, and fin $\alpha' =$ fin $\alpha - 2$ fin α'^2 cof. α' cof. $\lambda . \frac{\phi-\lambda}{\omega} . \delta$, by theorem, page 17; or becaufe $\frac{\phi-\lambda}{\omega} =$ cof. λ cotang. α feré, the fin α' is $=$ fin α — 2 fin α' cof. α'^2 cof. $\lambda^2 \delta$; confequently ω is $=$

$$\frac{d \text{ fin } \alpha}{\text{cof. } \phi} \left\{ 1 - \delta . \frac{\text{fin } \lambda^2}{\text{fin } \alpha^2} \right\} \times \left\{ 1 + \frac{2 \text{ cof. } \lambda^2 \text{ cof. } \alpha^4}{\text{fin } \alpha^2} . \delta \right\}.$$

From the theorem fin $\alpha' =$ fin $\alpha - 2$ fin α' cof. α'^2 cof. $\lambda^2 . \delta$, it is manifeft that α is nearly equal to $\alpha' +$ fin $2\alpha'$ cof. $\lambda^2 . \delta$.

When α is nearly equal to a right angle, we have $\omega =$ $\frac{d \text{ fin } \alpha}{\text{cof. } \phi} \left\{ 1 - \delta \text{ fin } \lambda^2 \right\} = \frac{D \text{ fin } \alpha}{R \text{ cof. } \phi}$, which correfponds with one of Legendre's theorems.

Prob. IV. Given two lats. and diff. long. To find horiz. angles.

4. 'Having given the latitudes of two places, and their difference of longitude, it is required to find the horizontal angles?'

Let λ, ϕ be the latitudes of two places, ω their difference of longitude, and α, β the horizontal angles at λ, ϕ refpectively on the fpheroid; alfo α, β the correfponding angles on the fphere, which may be found from the data by the rules of fpherical trigonometry. Then by the theorem, page 17, if we put,

$$M = 2 \text{ fin } \alpha'^2 \times \frac{\text{cof. } \lambda}{\text{cof. } \phi} \times \frac{\text{fin } \phi - \text{fin } \lambda}{\text{fin } \omega} = 2 \text{ fin } \alpha'^2 \times$$

$$\frac{\text{cof. } \lambda \text{ cof. } \frac{\lambda+\phi}{2}}{\text{cof. } \phi} \times \frac{\phi-\lambda}{\omega} \text{ feré,}$$

$$N = M. \left\{ \text{fin } \lambda \left(\text{fin } \lambda + \text{fin } \phi\right) - \frac{1}{2} \right\} + \text{cotang. } \alpha' \, M^2, \text{ and}$$

$$N' = M. \left\{ \frac{1}{2} - \text{fin } \phi \left(\text{fin } \lambda + \text{fin } \phi\right) \right\} + \text{cotang. } \beta' \, M^2,$$

we fhall have $\alpha = \alpha' + M\delta + N\delta^2$, and $\beta = \beta' - M\delta + N'\delta^2$.

Example. Let a $= 3496740$, b $= 3477210$, $\lambda = 49^\circ\ 40'$, $\phi = 50^\circ\ 0'$, and $\omega = 30'$. *(See the Account of the Trigonometrical Survey, &c. Vol. 1, Page 153).* Then the two colatitudes, and the included angle 30′, will give the fpherical angles α', β', 43° 51′ 48″,3, and 135° 45′ 16″,2 refpectively. The remaining part of the calculation is as follows:

2 fin

$2\sin\alpha'^2$ - Log. 9.98242	Sin $\lambda(\sin\lambda+\sin\phi)-\frac{1}{2}$	0.665036	
Cos. λ - - Log. 9.81106	Cotang. α'. M - -	0.432644	
Cos. $\frac{\lambda+\phi}{2}$ Log. 9.80957	Cotang. β'. M - - -	0.426906	
Sec. ϕ - - Log. 0.19193			
$\phi-\lambda$ - - Log. 1.30103	N - Log.	9.65937	$-$N′ Log. 9.65937
ω cour. - Log. 8.52288	206264″,8 - Log.	5.31443	
	δ^2 - Log.	5.49408	
M - - - - Log. 9.61889			
206264″,8 Log. 5.31443	M″δ^2 - Log.	0.46783	$-2''$,937
δ - - - - - Log. 7.74704			
M″δ - - - Log. 2.68036	479″,025		
	+2 ,937		
	481″,962 = 8′ 1″,962 = $\alpha-\alpha'=\beta'-\beta$.		

Therefore $\alpha = 43^\circ\ 59'\ 50'',262$, and $\beta = 135^\circ\ 37'\ 14'',238$.
Mr. Dalby makes $\alpha = 43\ \ 59\ \ 51\ ,\ 55$, and $\beta = 135\ \ 37\ \ 12\ ,\ 95$.

To ascertain the probable error of the theorem; that the sum of the horiz. angles on the sphere and spheroid are nearly equal.

I have already remarked, that the sum of the horizontal angles on the sphere and spheroid are very nearly equal, and that they would be perfectly so, if we were permitted to reject the terms of the formulæ involving the powers of δ higher than the first. We shall now, by retaining the square of δ, ascertain the probable error of this theorem.

We have then by the formulæ, $\alpha+\beta=\alpha'+\beta'+(N+N')\delta^2$.

Now N+N′ is equal to M $\left\{\sin\lambda^2-\sin\phi^2+\frac{\sin(\alpha'+\beta')M}{\sin\alpha'\sin\beta'}\right\}$

$=M(\sin\lambda-\sin\phi).\left\{\sin\lambda-\sin\phi-\frac{2\sin(\alpha'+\beta')}{\sin\omega}\right\}$; but in all the cases that occur in practice, λ and ϕ are nearly equal, and the sum of α', β' differs little from two right angles, wherefore the $\sin\lambda^2-\sin\phi^2$, and $\frac{\sin(\alpha'+\beta')M}{\sin\alpha'\sin\beta'}$ must be small, and the sum of these is not only to be multiplied by M, which is also small, but by δ^2, which is about $\frac{1}{90000}$. So that $(N+N')\delta^2$ is insensible, and therefore $\alpha+\beta=\alpha'+\beta'$.

When ϕ is nearly equal to λ, the formula may be consider-

ably

ably fimplified. For then $M = 2 \text{ fin } \alpha'^2 \text{ cof. } \lambda \times \frac{\phi - \lambda}{\omega}$, and $N = M (2 \text{ fin } \lambda^2 - \frac{1}{2}) + \text{cotang. } \alpha' M^2$; and if $\alpha = 90°$ ferè, M is $= 2 \text{ cof. } \lambda + \frac{\phi - \lambda}{\omega}$, and $N = M (2 \text{ fin } \lambda^2 - \frac{1}{2})$.

The particular ftates of the data, when the term involving the fecond power of δ is rigoroufly equal to nothing, may be thus determined:

In the firft place, if $\delta = o$, or the eccentricity of the fpheroid be infinitely fmall, α, α', and β, β', are exactly equal to each other.

Secondly, when $M = o$, N is $= o$, but M is $= o$ when $\phi = \lambda$, or the fine $\alpha' = o$, that is when the triangle is ifofceles, or the directions of the places due north or fouth of each other.

It is almoft rigoroufly exact. It appears then that this property of fpheroidal triangles, firft advanced by Mr. Dalby, and objected to by Mr. Playfair, is almoft rigoroufly exact; and it might eafily be fhewn, that its application will never occafion any material error, even in the moft unfavourable cafe that can be propofed. And it is not merely an elegant and curious theorem, but is highly valuable, as affording a method of determining the longitudes of places from terreftrial meafurements, almoft independent of all hypothefis. For whether the earth be an exact ellipfoid or not, any fmall portion of its furface may certainly, without error, be confidered as pertaining to one of fmall eccentricity, which fuppofition is all that is neceffary for demonftrating the theorem.

Determination of the eccentricity at place of obf. Our folution alfo affords an eafy method of determining the eccentricity at the place of obfervation. For if we have the latitudes and difference of longitude given, we fhall alfo have the horizontal angles on the fphere. But from obfervations of the pole ftar, we may find the horizontal angles on the fpheroid, and confequently the difference between them; but this difference is equal to a certain function of δ in our folution, whence we fhall have an equation, from which δ may be determined. Thus if α be the obferved horizontal angle on the fpheroid, and α' the computed one on the fphere, we have $M\delta + N\delta^2 = \alpha - \alpha'$. Now if we reject the term $N\delta^2$ as infenfible, we obtain a near value of $\delta = \frac{\alpha - \alpha'}{M}$, which fubftituted for δ in

$M + \delta$, gives $\delta = \dfrac{\alpha - \alpha'}{M + \dfrac{\alpha - \alpha'}{M}}$, very nearly. Here a queſtion naturally ariſes; *viz.* To determine the value of α' ſuch that δ may be obtained in this manner with the higheſt degree of accuracy, of which the method is ſuſceptible.

This will evidently be the caſe when M is about its maximum value. So that if we put the differential of $M = o$, and ſubſtitute the value of the differential of α' reſulting from the properties of the ſpherical triangle in this equation, we ſhall have the required value of the horizontal angle. Let us ſuppoſe λ and ω given, and there will reſult $\text{ſin}\ \alpha'^2 \times \dfrac{\text{ſin}\ \phi - \text{ſin}\ \lambda}{\text{coſ.}\ \phi}$ a maximum; whence 2 cotang. α' coſ. ϕ (ſin ϕ — ſin λ) $d\alpha' + (1 - \text{ſin}\ \lambda\ \text{ſin}\ \phi) \times d\phi = o$. But by ſpherics,

$$\text{Cotang.}\ \alpha' = \frac{\text{coſ.}\ \lambda\ \text{tang.}\ \phi - \text{ſin}\ \lambda\ \text{coſ.}\ \omega}{\text{ſin}\ \omega},$$

and $d\alpha' = -\dfrac{\text{ſin}\ \alpha'^2\ \text{coſ.}\ \lambda}{\text{coſ.}\ \phi^2\ \text{ſin}\ \omega} \times d\phi$; wherefore by ſubſtitution,

$$2\ \text{ſin}\ \alpha'\ \text{coſ.}\ \alpha' = \frac{\text{coſ.}\ \phi\ (1 - \text{ſin}\ \lambda\ \text{ſin}\ \phi)}{\text{coſ.}\ \lambda} \times \frac{\text{ſin}\ \omega}{\text{ſin}\ \phi - \text{ſin}\ \lambda}.$$

Alſo when ω and $\phi - \lambda$ are ſmall, the ſin $\omega = \dfrac{\text{tang.}\ \alpha'}{\text{coſ.}\ \phi}$. $\left\{\text{ſin}\ (\phi - \lambda) + 2\ \text{ſin}\ \lambda \times \text{coſ.}\ \phi\ \text{ſin}\ \frac{1}{2}\ \omega^2\right\}$, and ſin ϕ — ſin λ $= 2\ \text{coſ.}\ \dfrac{\phi + \lambda}{2} \times \text{ſin}\ \dfrac{\phi - \lambda}{2}$; conſequently $2\ \text{coſ.}\ \alpha'^2 =$

$$\frac{(1 - \text{ſin}\ \phi\ \text{ſin}\ \lambda)}{2\ \text{coſ.}\ \lambda\ \text{coſ.}\left(\dfrac{\lambda + \phi}{2}\right)} \times \frac{\text{ſin}\ (\phi - \lambda) + 2\ \text{ſin}\ \lambda\ \text{coſ.}\ \phi\ \text{ſin}\ \frac{1}{2}\ \omega^2}{2\ \text{ſin}\left(\dfrac{\phi - \lambda}{2}\right)},$$

and $2\ \text{coſ.}\ \alpha'^2 = 1$ if we ſuppoſe $\phi = \lambda$ nearly, and ſin $\frac{1}{2}\ \omega^2$ very ſmall.

When the horizontal angle α', therefore, is equal to 45° or 135°, the obſervations will be nearly in their moſt favourable ſtate for determining the compreſſion by means of our theorem. We might illuſtrate this method by examples taken from the trigonometrical ſurvey of Great Britain, but on reference to it I have found fewer complete ſets of obſervations than might be expected, and ſuch as are complete in every reſpect, are not well calculated for this purpoſe, the horizontal angles being

ing nearly right ones. The obſervations at Beachy Head and Dunnoſe give $\frac{1}{148,1}$ for the compreſſion; but it muſt be remarked, that the ſtate of the data is very unfavourable in this example.

The rules which our ſolution gives for computing the horizontal angles from the latitudes and difference of longitude, will be found, I apprehend, much ſhorter than Mr. Dalby's, beſides the advantage they poſſeſs of affording us the means of aſcertaining the figure of the earth by a very ſimple proceſs, from obſervations made with the ſame inſtruments and by the ſame obſervers.

The preceding rules applicable to an ellipſoid; but the managers of the trigonom. ſurvey avoid this aſſumption.

The theorems we have been detailing, with ſome others which may perhaps form the ſubject of another letter, would give us the relative poſition of one place to another on the ſurface of the earth, were its figure an ellipſoid of known dimenſions; but as this is ſtill conſidered as problematical, the method adopted by the gentlemen who have ſo ably conducted the ſurvey of our iſland, is certainly preferable. They firſt obtain the length of a degree upon the meridian, and its perpendicular in a given latitude, and employ theſe data for computing the geographical ſituations of all the places near that parallel, and not far diſtant from a known meridian. In the ſmaller triangles the truth may be thus obtained to the fraction of a ſecond, and in the larger ones they have very ſucceſsfully employed the beautiful property of ſpheroidal triangles, which we have ſo often mentioned.

How to apply the rules.

But though we give the preference to their method of computation, I conceive the preceding rules will be found equally accurate, if we make uſe of the values of c, a, and δ deduced from their obſervations; or if we aſſume near values of them, and note the agreement or diſagreement of the computations with obſervations made at a place conſiderably diſtant from the firſt ſtation. We may thus aſcertain nearly the error of our ſuppoſitions, and then correct the intermediate ſtations. This cautious method of proceeding is rendered neceſſary by the anomalies which have been diſcovered in the meaſures of degrees in different latitudes, as well as by the general rule, which ought to be our guide in all philoſophical inquiries, to frame as few hypotheſes as poſſible, but to make accurate experiments, and infer the truth from them by fair and genuine induction.

I mean

I mean not, however, to support the opinion, that the earth is not an ellipsoid; but, on the contrary, should be very loth to be obliged to give up an hypothesis, which is so beautiful in theory, and has stood its ground so long. Many of our objections to it may very probably arise from errors in observations, or from other causes which have not yet been fully examined. The remarks of Mr. Playfair on this subject are very ingenious, and I hope will be confirmed by the phenomena: but if not, I am convinced we have not the plea of inaccuracy to set up in this instance. One of the strongest objections, however, has been lately done away. The degree of the earth measured in Lapland in the year 1736, has been found, by some Swedish gentlemen sent there for that purpose, to err in excess by no less than 208 toises. Now if we advert to the number and character of the astronomers who originally measured this degree, it will be difficult for us to set limits to the errors of other observers. Perhaps the anomaly in the degree at the Cape of Good Hope arises from the same cause.

The earth probably an ellipsoid; particularly as the error of the Lapland degree removes much of the doubts entertained on this subject.

Fortunately, however, the great improvements, which have since been made in the instruments for astronomical observations and geodætical mensuration, afford us the means of bringing the probable errors of observation within very narrow limits. We may thus obtain a number of measures in different latitudes of equal accuracy, and by comparing them together, the question about the earth's figure may be put beyond a doubt. If this comparison shall be found to give different ellipses, we shall then be fully warranted in rejecting the hypothesis entirely and for ever. But till this is done, we may be allowed to adopt an hypothesis, which is so simple, so good in theory, and supported by so many strong arguments and accurate observations.

Late improvements will clear up this matter.

We have already remarked, that the degree lately measured in the Mysore, compared with that in France and England, gives $\frac{1}{300}$ for the compression at the poles: the corrected degree in Lapland gives $\frac{1}{313}$, and that measured in Peru, $\frac{1}{312}$. There is a considerable difference between the compressions deduced from other measures, but the mean falls between these limits. From the best observations of the length of the pendulum that swings seconds in different latitudes, the same conclusion is also drawn; the second pendulum near the pole compared

Mean compression from measurement; and from the pendulum;

compared with that at the equator, gives $\frac{1}{304}$ for the compression. We may therefore assume $\frac{1}{304}$ as being very nearly its true value.

from celestial irregularities; It is a curious circumstance to find the figure of the earth, deduced from the measurement of lines and angles on its surface, confirmed (perhaps corrected), by observations of the stars and planetary bodies in the heavens, combined with the theory of universal gravitation. But such is certainly the case.

e. g. of the moon. Among others may be mentioned two small inequalities in the moon's motion, which the industry of modern mathematicians have unfolded. One of them was first taken notice of by Mayer, and fixed by Mason at 7″,7, but was neglected by astronomers, as it did not sufficiently appear that such an equation should arise from the theory, till Laplace traced it to the oblateness of the earth's figure. Its argument is the longitude of the moon's node, and its value has been found by Burg, from the observations of Dr. Maskelyne, to be equal to 6″,8, which answers to a compression of $\frac{1}{305{,}05}$. There is also another inequality of the moon's motion in latitude, which depends on the sine of the true longitude, and results from a nutation in the lunar orbit, produced by the action of the terrestrial spheroid. Burg has also determined the coefficient of this inequality, from a great number of observations, to be equal to 8″,0, which results from a compression of $\frac{1}{304{,}6}$.

Remarks upon Newton's discovery and investigation of this subject. The precession of the equinoxes, and the nutation of the earth's axis, were discovered by Newton to arise from the oblateness of the earth's figure. This famous problem is acknowledged to be one of the most abstruse in physical astronomy, and its complete solution requires the utmost resources of the modern analysis. The compression thence arising is equal to $\frac{1}{304}$, agreeing exactly with the results from the two lunar inequalities, the lengths of the second pendulum, and the best measurements on the earth's surface. It is well known that Newton failed in attempting to solve this problem, and some French mathematicians have been disposed to pride themselves on being the first to detect it. It ought however to be remembered, for the honour of that great man, that his mistake did not arise from any error in principle, but from his taking for granted, without demonstration, a circumstance which appears highly probable, but is really erroneous. He seems to have

have first made this assumption in order to shorten his solution, and on finding the calculations to agree with observation, to have never after returned to the subject. The mathematicians of his time were unwilling or unable to follow him, and the question remained as it came from his hands, till the middle of the last century. Had doubts arisen and objections been started, the genius of Newton might have been once more roused to action, and continued to enlighten the sciences to the last. But unfortunately no such incentive was given, and Newton stopped short in the career of his glory. The evening of his life was devoted to other studies; but however usefully he may have been employed, there are few who will not be inclined to lament that he ever laboured in the Mint or the Revelations.

Thus much I have thought it not entirely foreign to observe on one of the most remarkable effects of the oblateness of the earth's figure, and in justice to its immortal discoverer, the inventor of the modern analysis, the father of physical astronomy, and the preceptor of Europe.

I am, Sir,

Your most obedient servant,

PEREGRINUS PROTEUS.

Portsmouth,
May 6, 1804.

Corrections to be made in the first Letter.

In page 12, line 20, after *paper* insert *but.* In page 15, line 23, &c. divide the coefficient of the term $\frac{c}{a}$ by cos. λ cos. ϕ; also in page 16, lines 3 and 12. In page 16, line 9, *dele* and cos. λ cos. ϕ'; and also in line 14. In page 17, line 6, &c. for cos. ω read sin ω. In page 18, line 23, for Pc, Qc, Rc, read $P.\frac{c}{a}$, $Q\frac{c}{a}$, $R\frac{c}{a}$.

IV.

Description of a striking Part for a Clock, in which the Intervals between Stroke and Stroke are not regulated by a Fly, but by a Pendulum. By Mr. EDWARD MASSEY.*

SIR,

New striking part for a clock.

HAVING for a number of years considered that a method of striking a clock, at certain regular intervals, might be of great service in making observations on the heavens, and ascertaining the velocity of sound, *&c.* I beg leave to lay before the Society for the Encouragement of Arts, *&c.* a striking part of an eight-day clock, which I have no doubt will answer the purpose intended; and if, upon examination, the Society should be of opinion that it may be useful, I trust they will reward it according to its merit. They will find that the work of this model is less than that of the common striking movements, and may be made by a common workman, with less expence and trouble; the weight required is also considerably less. The principle I act upon is the pendulum, by which I regulate the stroke, instead of the fly; the advantage of which must be obvious to every one. The machine consists of a toothed wheel A, one pinion B, a pin wheel C, pallets D D, pendulum E, and locking detent G. The hammer-work F is as usual, and strikes on the bell at H. The weight hangs to the cord I. See *Plate* XII. *Fig.* 1 and 2, where a front and side view of the machinery are given, and where similar letters denote the same parts in each view.

Description of the Machinery.

I consider it is only necessary for me to give the description of the wheels, so as to be a direction to a mechanic, who wishes to manufacture clocks on this principle. The main-wheel A, with seventy-eight teeth, is to act in a pinion of eight leaves, B. The pin wheel C should be large, so that the pins on which the pallets D and the locking G act, may be flung as far from the center as possible, which pins may be eight or sixteen in

* From the Transactions of the Society of Arts, 1803. This invention was honoured with a reward of 20 guineas.

number,

number. If eight, the pendulum E ſhould be about nine inches long, and it will vibrate twice betwixt each blow of the hammer; but if ſixteen pins are put in the wheel, the pendulum muſt be about three inches long, and will make four vibrations betwixt each blow. The pins for drawing the hammer muſt be eight in number, and be fixed in a circle of about half the diameter of the aforeſaid pins. The locking-plate is on the main wheel. The ſtop is againſt the pins on which the pallets act, and may be diſcharged by a flirt piece. New ſtriking part for a clock.

As I have deſcribed the model, I beg leave to point out the method of ſtriking a clock by the common pendulum, true ſeconds, without any additional pendulum or pallets for the ſtriking part.

Deſcription.

Fix a cantrite wheel with ſixty teeth on the ſame arbor with a ſwing wheel of thirty teeth. Now ſuppoſe a ſtriking part to be made in the common way of making an eight-day clock, ſo far as the pallet pinion, leaving out the warning and fly pinions. A crank piece muſt be fixed on the pallet pinion, which muſt come into contact with the cantrite wheel, which is fixed on the ſwing wheel arbor. Then ſuppoſe the clock to be ſet a going, and the rack diſcharged, the pallet pinion will make a revolution on every vibration of the pendulum, by which means a clock will ſtrike ſeconds as true as a pendulum vibrates, which I hope will be conſidered as uſeful for the purpoſes I have deſcribed. I alſo beg leave to obſerve, that a great advantage ariſes in both the above machines, from their not being liable to foul, as the ſtroke is given by the certain and regular vibration, inſtead of the uncertain motion of the fly. Its advantage likewiſe depends on the cleanneſs of the work; and church clocks will be much benefited from the decreaſe of weight.

I am, Sir,

Your moſt obedient ſervant,

EDWARD MASSEY.

Charles Taylor, Eſq.

Hanley, in Staffordſhire,
Jan. 12, 1803.

V.

Deſcription of a very ſimple Telegraph, conſiſting of the Human Figure adapted to converſe at a Diſtance by means of Signs *.

Preliminary obſervations.

AMONG the great advantages of which thoſe who do not enjoy ſight are deprived, we may reckon telegraphy, which it would be difficult to ſupply by another ſenſe. Such an art for the uſe of the blind would doubtleſs be very imperfect. On the contrary, the advantages which it affords to thoſe who enjoy light, appear to me to be ſo important, that it ought to be rendered more general, and brought within the reach of all men. For this purpoſe I have offered ſome notions on the ſubject. I do not pretend either to be the firſt, or the only one who has entertained them: I even believe the contrary; and, in this inſtance, they will have that in common with telegraphy in general, which, though new in its execution, was not ſo in its invention †.

* From a ſmall pamphlet in French, extracted from *Mémoires ſur les Aveugles*, &c.

† It is not extraordinary that the ſame invention ſhould have been thought of by ſeveral perſons. Thus the notion of telegraphy may be found in the preface to one of the German works of the celebrated Chr. Louis Hoffman, a native of Rheda, and phyſician to the Elector of Mentz, which, however, does not leſſen the merit of the French inventor. A deſcription of the telegraph invented and executed by Citizen Chappe, is found in the intereſting work of Mr. Meyer, intituled, *Fragmente aus Paris, im. IV ten Jahr der Franzœſiſchen Republik*; *Hamburg*, 1797. The author, who was in the telegraph office at the Louvre with Cit. Chappe, affirms, that the latter had made his diſcovery before the revolution, that he communicated it to the National Aſſembly in 1792, and that the Convention, on the report of Lakanal, decreed, July 25, 1793, the eſtabliſhment of a telegraphic correſpondence, under the direction of Citizen Chappe, as telegraphic engineer. A notion may be formed of the rapidity of the telegraphic correſpondence, by the following example, of which Mr. Meyer was a witneſs. He ſays, that during his preſence in the office at the Louvre, and at the appointed hour in the evening, enquiry was made of the office at Liſle, *by a ſingle ſignal*, if any thing new had happened in the army of the north, and the anſwer, no, was received in 88 ſeconds.

Perſuaded of the importance of communicating our ideas at diſtances too great for the voice or hearing, I have employed myſelf in enquiries for a telegraph which ſhould be at once cheap and ſufficiently perfect to be eaſily uſed. I believe I have diſcovered it; nature herſelf has given it to all the world. This telegraph is the human body, its branches are the arms, which, with each other and with the perpendicular line of the trunk, may form a great number of figures, ſufficiently diſtinct to be readily ſeen, at conſiderable diſtances, by ſimple viſion or by the aſſiſtance of a teleſcope. It would certainly be very agreeable for two friends, living oppoſite to each other in an extenſive place or on the banks of a large river, to be able to converſe together. Of what utility might it be to the inhabitants of open countries to have a method of communication which is at once ſpeedy and requires no expence!

Natural telegraph.

I hope, therefore, that the generality of readers will ſee with pleaſure that a method of communication is opened to them, ſuſceptible of being varied and brought to perfection by themſelves. In the annexed *Plate* X. are found the ſigns for all the characters in the alphabet, for the figures, and for the punctuation. To ſimplify the writing of theſe ſigns, the perpendicular and immoveable line, which repreſents the trunk of the body, may be omitted, as I have done, only indicating it in ſome ſigns or characters by a point, as in the *e* and *u*; and to write them with more rapidity, they may perhaps be joined in the manner of ſhort-hand writers.

Method of writing the ſignals.

Three different poſitions of the right arm, and as many of the other, form the ſigns for the vowels. The right arm ſtretched out, and a little raiſed, forming an angle of about 45° with the line of the trunk, gives the ſign which expreſſes *a*; the ſame arm extended and more elevated, or horizontal, forming a right angle with the trunk, gives *e*; more elevated, and forming an obtuſe angle of about 135°, it furniſhes *i*. The left arm extended, and forming an angle of about 45° with the trunk, gives us the ſign for *o*; more elevated, or horizontal, it ſignifies *u*; ſtill more elevated, and forming an angle of about 135°, it gives *y*.

Telegraphic ſigns by the human figure.

It may be obſerved, were it only to aſſiſt the memory, that *a* and *o*, whoſe ſounds have ſome reſemblance in ſeveral words, are indicated by the ſame ſign, and likewiſe *i* and *y*, which in the French language often expreſs but a ſingle ſound; with

this

Telegraphic ſigns by the human figure.

this difference, however, that the ſigns for *a* and *i* are formed by the right arm, and thoſe for *o* and *y* by the left arm. The two other vowels, *e* and *u*, are likewiſe indicated by the ſame ſign, but with the ſame difference.

To form *b*, both arms deſcribe an angle of 45°. To form *c* and *d*, the right arm is kept in the ſame poſition, and the left arm is raiſed to the height of 90° for the firſt, and of 135° for the latter.

To form the letters *f*, *g*, and *h*, the right arm is extended horizontally, and the three different angles of 45°, 90°, and 135°, are formed with the left arm.

To expreſs *j*, *k* or *q*, and *l*, the right arm is raiſed to the height of 135°, and the ſame poſitions are repeated with the left arm as were employed for the ſix preceding conſonants.

To form *m*, the upper part of the right arm is placed in a horizontal line, and the form-arm is raiſed at the ſame time, ſo as to form a right angle with this part. To deſignate *n*, the left arm is placed in the ſame poſition. To form *p*, both arms deſcribe the preceding figure at the ſame time.

To expreſs the letters *r*, *s*, *t*, the upper part of the right arm is placed in an horizontal line, and a right angle is formed with the fore-arm, while the extended left arm ſucceſſively deſcribes the three different angles of 45°, 90°, and 135°.

For *v*, *x*, and *z*, the ſame is done with the left arm as was done, for the three former letters, by the right, which then forms the three angles of 45°, 90°, and 135°.

If it is intended to ſhew that a telegraphic ſignal is terminated, the two arms are withdrawn, ſo as to form but one line with the reſt of the body.

The right arm put at reſt, ſo that the hand is ſupported by the hip, indicates the termination of a word.

The left hand placed in the ſame poſition, is the ſign of a comma (,).

The two arms put in this poſition, ſignify a point and a comma, (;).

By putting the right arm into this poſition, and at the ſame time making the ſign of *n* with the left hand, two points are indicated, (:).

By reſting the left arm on the hip, and making the ſign of *n* with the right arm, a point is indicated, (.).

By

Telegraphic signs by the human figure.

By holding the right arm in ſuch a poſition that the hand ſhall be above the head, or touch it, the point of interrogation is made, (?).

The ſame motion performed with the left arm, furniſhes a ſign for the point of admiration, (!).

The three ſigns of the termination of a word, of a comma, and of the point of interrogation, each combined with the three different angles made with the other arm, form the nine figures.

By putting the right arm at reſt on the hip, and forming the three angles of 45°, 90°, and 135°, with the extended left arm, the ſigns are given for the figures 1, 2, and 3.

By reſting the left arm on the hip, and making the ſame three angles with the right arm, the figures 4, 5, and 6, are formed.

By bringing the right arm to the ſign of the point of interrogation, and repeating the three angles with the left arm, the figures 7, 8, and 9, are formed.

To indicate zero, (0), the two arms are raiſed ſo that the hands ſhall be above the head, or touch it.

To expreſs 10, the ſigns of 1 and 0 are made without making either the ſign of the termination of a word, or of a number, or that of a comma, between theſe two ſigns. To expreſs 11, the ſign of 1 is twice made, with the aſſiſtance of the ſign for the termination of a character, or of a figure. To indicate 12, the ſigns of 1 and 2 are made, and ſo on: 10, 11, and 12 may be expreſſed at pleaſure, each by a ſingle ſign, by placing the left arm in the poſition of the point of admiration, and by ſucceſſively forming with the right arm the three angles of 45°, 90°, and 135°. It would not be difficult to deviſe a great number of ſigns to expreſs 13, 14, 15, and many other numbers, each by a ſingle ſign.

Mechanical addition for great diſtances.

If it be intended to make uſe of this natural telegraph at the diſtance of one, or even of ſeveral leagues, or to do without a teleſcope at leſs conſiderable diſtances, it is only neceſſary to add to each natural arm an artificial one, that is to ſay, an oblong frame, of the breadth of a foot or thereabout, and of the length of one or more yards. This frame muſt be covered with oiled ſilk, or any other ſtuff of a dark colour, and muſt be provided with a handle to hold and direct it. Our telegraph would here no longer be called *natural*, but it is ſtill among

the

the most ſimple of the artificial. I am of opinion that this kind of telegraph may be employed with very great advantage: it is ſimple and cheap; it may be uſed in all directions, and is removed with eaſe. In this latter point of view, it would doubtleſs be well calculated for the formation of moveable telegraphic lines, or what might be called a flying telegraphy, which in my opinion would be very uſeful in war, to keep up a quick and conſtant communication between the different bodies, and with the fixed and common telegraphic lines. A machine of eaſy carriage, and calculated to raiſe a man to a conſiderable height, would, no doubt, contribute to render the flying telegraphy more perfect. The telegraphs with the frames may be uſed by night as well as by day, by adding lanterns to them, as in the common telegraphs. One muſt be placed in the middle, that is to ſay, on the breaſt of the perſon who forms the telegraphic ſignals with his arms and the frames, one at the handle, and one at the extremity of each frame. To prevent the perſon from being affected by the vapours of the lantern in the middle, it ſhould be placed at a ſmall diſtance before him; and when the movements are intended to be viſible on both ſides, it would be ſtill better to place this lantern on the head, or to put one on each ſhoulder.

May be uſed by night as well as by day.

Perhaps this kind of telegraph might be advantageouſly employed to eſtabliſh immoveable telegraphic lines, in every caſe in which the telegraphs employed at preſent would be too expenſive. The ſame telegraph might frequently ſerve for ſeveral lines in different directions. By means of this telegraph a communication might be maintained, with little coſt, even in ſmall diſtricts, or in the provinces and departments, with the capital or the chief place, and the other conſiderable points. Of what importance may it be to be able to indicate inſtantaneouſly conflagrations, inundations, or other events which require ſpeedy ſuccour.

It would be well to place the telegraph, or the perſon who makes the telegraphic ſignals, on a kind of ſtool, to facilitate his movements. Placed in this manner, with his arms and the frames he could make a prodigious number of figures, ſufficiently diſtinct not to be confounded with each other.

Account

VI.

Account of a Fact respecting Water heated in a Boiler of Stone; with Observations. By Mr. J. C. HORNBLOWER.

To Mr. NICHOLSON.

DEAR SIR,

THOUGH it must be admitted by every one who has attended to the critique contained in your note on the experiments of Count Rumford, in your first volume * of the Philosophical Journal, concerning the non-conducting power of fluids with regard to heat, that it is an erroneous conclusion, and which several experiments, particularly that of Mr. Murray, have since confirmed; yet I cannot be quiet without informing the gentlemen who have so constantly opposed the Count's doctrine, of a circumstance which accident brought forth on a very large scale, about forty years ago, in the county of Cornwall.

Fluids not strictly non-conductors.

Incident.

It had long been a desideratum in the mining interest in that county, to reduce the consumption of fuel in draining the mines by the means of steam engines, and every expedient that carried any tolerable face of probability was brought to a trial.

Attempt to reduce the consumption of fuel in working steam engines.

Among the rest it was suggested, that as, in the several operations of smelting the produce of the mines, much heat must be carried off, from the intense fires of their furnaces, which might be employed to some purposes requiring but a subordinate temperature; a resolution was formed by a company of gentlemen, with Mr. John Weston at the head of it (to whom mining was an ungovernable hobby-horse), to erect an engine on a copper mine in the parish of Camborne, and to put up a set of furnaces so attached to the engine that they might avail themselves of this superabundant heat to raise the steam.

Trial to work by the smelting heat.

To effect this they had their engine-boiler made of masonry, of what is called in Cornwall *moor-stone*, well wrought and put together with Aberthaw lime, which has the property of setting

A stone boiler connected with water-lime, &c. heated by copper tubes.

* I. Page 291, Quarto Series.

like

like the Dutch trafs *; and to convey the heat, two or three copper tubes were placed in it from end to end, and the furnaces connected to one end of the boiler, and the engine at the other; which, though it was a prodigal way of faving heat, yet it was competent enough to raife fteam for the ufe of the engine in certain cafes.

When the water boiled, a cock near the bottom gave water, but little heated.

At the bottom of the boiler was placed a cock, as ufual, to tap it as occafion required, either when it was neceffary to clean it, or in cafe it fhould be over-charged with water, which I think was the cafe which brought the following fact to light; namely, that when the fire had been lighted, and the heat had to circulate through all this mafs of ftone-work, and the water brought to boil, and the engine had been at work for the firft time, which is ufually attended with many unforefeen delays and hindrances, the cock being turned, the water was not hotter than to admit the hand without any painful fenfation of heat. Many deep and grave hypothefes were formed on this extraordinary difcovery, which, for the fake of the profeffion at this advanced ftate of fcience, I muft forbear to mention. But at that period many a valuable truth had come and gone like a worthlefs mendicant, whom few regarded, and whom none would receive.

The tubes were near the furface, and the veffel a bad conductor.

The moft ftriking circumftances in the detail of this difcovery are, that the temperature downward (for the tubes were near the furface of the water) muft be greatly accelerated by the conducting power of the fides of the boiler, and the agitation of the water in a ftate of ebullition: notwithftanding all, it muft have occupied four or five hours to bring the lower part of the water to the temperature of 90°, or perhaps 100°; and no doubt can be entertained but that they deemed it a matter of no confequence as to the place of the tubes, fo long as they were covered with water; believing with every body elfe, that the whole mafs would attain the boiling point at the fame time.

Hail the man who throws if but one grain of truth into the fcale againft popular prejudice or vulgar error; and notwithftanding the Count did not obtain the extreme of precifion in this particular inftance, he has laid the foundation for all the fubfequent experiments which have gone to confirm or reject

* This lime is found on the beach on fome part of South Wales.

the

the position he advanced. This I may be permitted to observe, without the imputation of detraction from those whose labours and talents demand my respect.

I am, Sir,

Your very obedient servant,

City Road, J. C. HORNBLOWER.

June 11, 1804.

VII.

Facts and Observations tending to elucidate the Theory of Galvanism. By A CORRESPONDENT.

General effects and theory of galvanism.

THE pile of Volta has disclosed many new properties which were not known to belong to the two powers of the electric fluid, for by its operation, two sets of chemical phenomena are produced, differing essentially from each other, thereby determining the real or positive nature of each power. It has introduced electricity as a chemical agent, and has shewn us that it acts as such in its two-fold character very generally throughout nature. Electricity has hitherto been considered mostly as an adventitious power excited upon the surfaces of bodies, it is now known to attach itself to, and to enter into combination with various substances.

If the spinal marrow of a frog be coated with tin foil, and the muscles of the lower extremities be laid bare, if the frog, thus prepared be drawn through water, at the bottom of which there is a piece of silver, when the tin foil touches the silver, the limbs of the frog are thrown into strong convulsions. If the tin foil be kept steadily at the first place of contact, the convulsions soon cease; but if it be removed to the smallest distance from where it first touched the silver, and is again in contact with it, the convulsions are renewed with their original violence. The same thing happens when the silver is connected with the muscles by means of a metal, though that metal touch the muscle in a point, as when the water acts as the conducting arch.

These experiments shew that there is a power in each assignable particle of the metal which excites the action of the muscles, and that nothing but the contact of the metals seems necessary for the production of the effect.

It

General effects and theory of galvanism.

If the power which excites the motion of the muscles be electricity, it is in a state not to be conducted from one particle to another, for the same effect is renewed whenever the tin-foil comes in contact with another part of the silver, however contiguous it be to the one at first touched. We find farther that the superficial particle does not take off the power of the one immediately beneath.

It is thus, I apprehend, that the renewal of surfaces gives a permanency to the powers of the pile, and in proportion to the quickness of succession in the presenting new surfaces, the power of the pile is found to increase.

It is needless to explain these appearances by the hypothetical principle of Mr. Lavoisier's oxygen, since Mr. Volta has shewn that the metals themselves are electro-motors, even without the intervention of water, or the action of acids or alkalis. Taking it for granted then that the animal muscles are a species of electrometer, we may conclude that some disturbance of electricity takes place when two metals are brought in contact, and a circuit is made. This disturbance varies according to the substances employed. Mr. Volta has found this to be the case, for he has rendered this small portion of electricity perceptible by accumulating it on his condenser. The pile of Volta accumulates the power which is called forth by the contact of two metals, and enables us to apply it in such accumulated state in various chemical experiments.

One of the most striking effects is the disengagement of two elastic aeriform fluids from water, by introducing wires, from which are produced the two states of the electric fluid. Thus it happens that the wire connected with the zinc produces, when immersed in water, one gas, and the wire connected with the silver or copper plate another gas. The properties of these gases are perfectly distinct, and evince that the producing powers have peculiar effects, for the water is common to both. What takes place from the wires in water at the extremities of the pile is more or less apparent between the plates themselves, where wetted paper is interposed, for there the action of the gases is to be seen in their effects on the metallic plates. The effect produced by the pile may be greatly increased by arranging it in the following order; silver, wet paper, zinc, a plate of glass, &c. and by placing wires on each side of the glass, and putting those into water; from each

pair

pair of wires the two airs will be produced. Might not Mr. Wilkinfon's experiment of burning charcoal be in this manner greatly increafed in fplendour? General effects and theory of galvanifm.

In the profecution of the prefent inquiry, I fhall efpecially notice the two different effects produced in water by the wires of Volta's pile. It is known that thefe two wires throw out two powers fimilar to what are called pofitive and negative electricity; and as water is common to both, I am led to conclude that the two gafes are produced in one inftance by the union of one of thefe powers with water, and in the other by the other power and water, and to confider each gas as thus compounded, becaufe we have no evidence of any other power, principle or fubftance being acceffory to the production of either of the gafes. Volta's pile caufes combuftion in atmofpheric air, and therefore vital air is abforbed; it caufes another fpecies of combuftion in vacuo, becaufe the two principles of fire are fupplied by it. Volta's pile does not act in vacuo, becaufe the refiftance neceffary to accumulate the powers is withdrawn in the fame manner as the Leyden jar does not charge in vacuo.

If the gas which is produced from one of the wires communicating with the pile in water be united and inflamed with the other in a juft proportion, the water which is common to both is re-produced, and common fire is generated in great abundance; now as we have had no evidence of the prefence of fire till this point of time, does it not appear that thefe two principles which are thrown out from the wires of the pile, are by their union transformed into ordinary fire, and does it not appear in this experiment that ordinary fire is generated by and compounded of thefe two powers?

Lavoifier has triumphed over the friends of Phlogifton. He overturned a fyftem which was built upon the affumption of a principle, upon a mere name of hypothetical entity, endued with attributed or imaginary properties. But in the place of the fyftem he deftroyed, he has erected one of his own. Has he not built upon the fame weak foundation with his late adverfaries? Does not his fyftem reft altogether upon the affumed diftinct exiftence of things, the diftinct exiftence of which cannot in any way be evinced? Do not all his folutions of chemical phenomena depend on fuch affumption? May not all thefe phenomena be explained with equal eafe, nay more fimply, by referring them to the action of known agents?

If

General effects and theory of galvanism.

If we examine Mr. Lavoisier's theory of combustion, we shall find that it depends upon the assumption of a principle whose existence has never been proved by direct experiment. It is the union of the basis of oxygen gas with the combustible body which in every instance is the cause of the combustion. We have seen that this gas is produced by positive electricity and water, and we conclude that the ponderable part of this gas is water, because we can perceive the agency of no other principle or substance. The simple condensation of oxygen gas, according to Mr. Lavoisier, occasions the evolution of heat and light. But little heat is excited by the mechanical condensation of this gas; but we find that when a chemical union takes place between a combustible and the gas, a great portion of heat and light is disengaged.

The accension of charcoal and metallic bodies by the galvanic battery, even in vacuo, seems to shew that the electricities thus produced are the peculiar agents in the phenomena of combustion. They seem not only to be the exciters of combustion, but they furnish with water those gases by which combustion has been conceived to be upheld.

The theory of the Lavoisierian school, of the combustion of hydrogen gas, in conjunction with oxygen gas, is as follows: "When both gases are mixed at a lower temperature than that of ignition or red heat, the attraction of their respective bases to the caloric is, in that case, stronger than their attraction to each other, and therefore they are not decomposed. But in the heat of ignition, their bases, namely *concrete* oxygen and hydrogen, again attract each other more strongly, and hence they unite to produce water; and both the caloric and light by which they are retained in their aerial form, are again disengaged from them, and constitute the fire."

Now where is the evidence of the heat and light being necessary to the formation of these gases, when they are produced from the wires connected with the extremities of the pile, and placed in water? Also during the oxidation of a metal in the moist way, the hydrogen discharged is at first very hot, so as to heat immoderately the tubes used in collecting it. If the condensation of oxygen gas be the cause of the production of heat and light, how comes it that charcoal and nitre deflagrate together with the production of so much light and heat, although the compound formed, namely, carbonic acid gas, occupies so much

much more ſpace than the ſubſtances from which it is formed? Does it not ſeem an aſſumption to ſay that the two conditions of oxygen in the aeriform ſtate, and oxygen in the ſolid ſtate, as in nitre, ſhould hold the ſame quantity preciſely of heat and light? This fact diſproves the opinion that the production of heat and light is commenſurate with the condenſation that takes place in combuſtion.

General effects and theory of galvaniſm.

An eſtabliſhed law of chemical affinity would be reverſed, if oxygen, when in combination with another ſubſtance, were to have a greater, or even the ſame attraction for the matter of heat as it has when oxygen is ſimply combined with the matter of heat.

There are many inſtances of combuſtion, deflagration, &c. wherein the compounds formed ſeem to hold more heat in their compoſition after than before combuſtion. When gun-powder is inflamed, does not the production of ſteam ſeem to ſhew that a great quantity of heat is *generated* during the exploſion. If the matter of heat was held to the particles of the component parts of gun-power, in conſequence of their great capacity for containing it, ſhould we have any reaſon by analogy to ſuſpect that the electric ſpark would entirely reverſe the order of the exiſting attractions? Have we not evidence of the accumulation of one power of the pile in nitrous and muriatic ſalts, for in the formation of their acids we do not find combuſtion to be neceſſary, and there is no known expenditure of this their igneous principle. Theſe are the ſalts which cauſe the combuſtion of inflammable bodies, and burn them, in conſequence of their both containing igneous and inflammable principles. Thus does it appear that fire is generated during combuſtion.

From all the obſervations and experiments I have made on this ſubject, I conclude that water is ſtill to be conſidered as a ſimple ſubſtance, that its two aſſumed component parts are non-entities, that fire is generated in every ſpecies of combuſtion, whether by acids or otherwiſe, and that heat is generated in the lungs during reſpiration. I take the liberty alſo to ſuggeſt, that the admiſſion of the hypothetical ſubſtances of the Lavoiſierian ſyſtem may have retarded the progreſs of ſcience, by diverting the mind from real objects. Analogies drawn from imaginary data muſt tend to perplex and confound, and thus do names ariſe without exiſting realities. Perhaps it would

would be as proper to queſtion the exiſtence of azote, if oxygen and hydrogen were to fail, as it was to admit it, from the ſuppoſition of the exiſtence of thoſe two ſubſtances.

A CORRESPONDENT.

VIII.

Experiments with the Electric Pile, by Mr. Ritter, of Jena. Communicated by Mr. ORSTED.*

WHILE the great inventor of the electric pile was proving the identity of galvaniſm and electricity, many of the philoſophers of Germany were buſied in the ſame purſuit. On this ſubject the celebrated Ritter undertook a very extenſive ſeries of experiments, the reſults of which are ſo remarkable as to merit attention, even after the publication of the labours of the philoſopher of Pavia.

To compare galvaniſm with electricity we muſt attend to the kind, chemical action, ſpark and ſhock.

In order to compare galvaniſm properly with electricity, four different phenomena are to be diſtinguiſhed; the kind of electricity, chemical action, ſpark, and ſhock.

1. Kind. The pile has a poſitive and a negative pole: and the intenſities decreaſe gradually from the exremities to the centre. But the whole pile becomes negative, when the poſitive pole is made to communicate with the earth by means of a conductor, and *vice verſa*. Yet this change in the kind of electricity does not affect its chemical action. A collateral fact is the increaſe of the action by adding a ſalt to the water without changing the kind of electricity.

As to the kind of electricity, it is known to every perſon, that the pile has two electrical poles, one poſitive, the other negative. On a more attentive examination we diſcover, what was not difficult to foreſee, that the reſpective intenſities are ſtrongeſt at the extremities of the pile, and regularly decreaſe from the extremity to the centre, where the intenſity is at its minimum. But it was never yet ſuſpected, that the whole pile becomes negatively electrified, when a communication is made between the poſitive pole and the earth, by means of ſome conducting ſubſtance; and on the contrary, the whole pile becomes poſitive; when the electricity is abſtracted from the negative pole. We have here a phenomenon, that ſhews the theory of electricity to be ſtill in its infancy: When the ſtate of the pile is thus changed, its chemical action is not deſtroyed, but continues as before. This fact is perfectly conſiſtent with the augmentation of the chemical action of the pile, on the addition of ſalts, acids, or alkalis, to their wet ſtrata, in which caſe the kind of electricity remains conſtantly the ſame as when ſimple water was employed.

* *Journal de Phyſique*, December, 1803, Vol. LVII, p. 401.

The

The affertion of Volta, fince repeated by Van Marum and Pfaff, that the electrical pile charges a jar or a battery inftantaneoufly, requires fome limitation.

Volta, Van Marum, and Pfaff, fay, that the pile changes a jar inftantaneoufly.

With regard to common piles, the affertion is true: but if the pieces of pafteboard, inftead of being thoroughly wetted, contain but very little moifture, that of the damp air for example, the action takes place much more flowly. At firft, indeed, the action is ftill tolerably prompt, but in proportion as the pafteboards lofe their moifture, the action becomes gradually flower; fo that a pile of fix hundred pairs of zinc and copper, ufed immediately after it was made, was ten or fifteen minutes charging a battery of thirty-fix fquare feet to the fame degree, to which it would have charged it inftantaneoufly, had it been conftructed with pieces of pafteboard thoroughly wetted. Each of the wet ftrata too may be compofed of a piece of glafs, armed on each fide with wet pafteboard; and fuch a pile of fix hundred pairs would require twelve hours to charge the battery to the fame degree, as would be done by a common pile, with a folution of a falt, in an imperceptible fpace of time. The law of this retardation therefore is, that the action of a pile is more flow, in proportion as it is a worfe conductor.

This is true with the common pile; but if the pafteboards be little moiftened, the jar will be charged more flowly.

Such a pile of 600 pairs was 10 or 15 minutes charging a battery of 36 feet as high as it would have done inftantaneoufly with thoroughly wet pafteboard.

By interpofing glafs armed with wet pafteboard between each pair of metals, the time would be protracted to twelve hours.

The action of the pile therefore is quick in proportion to its conducting power.

Ritter has made a great number of experiments in particular, which prove, that the ftate of the pile, *on all occafions*, obeys the fame laws, as that produced by the electrical machine: but we cannot here enter into particulars, without exceeding the bounds allowable to an abftract.

Ritter has proved by many experiments that the electricity of the pile obeys the fame laws with refpect to kind as that produced by the machine.

It is a well known fact, that electricity produces the fame change in water as galvanifm. Ritter has fhown, that pofitive electricity, like pofitive galvanifm, difengages from it oxigen gas; and that negative electricity, like negative galvanifm, difengages from it hidrogen.

2. Chemical action.

Galvanifm and electricity produce the fame change in water; each in the pofitive ftate extricates from it oxigen; in the negatives, hidrogen.

Inquiries into the action of the pile on metals have taught him, that its negative pole difpofes them to combine with the hidrogen of water, as the pofitive pole difpofes them to combine with oxigen. The hidrogenation has different degrees with refpect to the fame metal, as well as the oxidation. Silver with a large quantity of hidrogen affumes the ftate of gas: with a fmaller quantity it remains folid. He alfo found, that the electricity does not produce oxigenations and hidrogenations in the humid way alone, but in the dry way alfo. The oxidation

The negative pole of the pile difpofes metals to

tion produced by the positive pole is easily observable. Nothing more is necessary, than to arm it with a leaf of gold, and the negative pile with a bit of charcoal; when, on bringing them into contact, the gold leaf will burn with a bright flame, and the charcoal remain untouched. If the charcoal be placed in contact with the positive pole, and the gold leaf in contact with the negative, the charcoal will burn, and the gold melt. The hidrogenation produced by the negative pole is less distinct, so that it is seldom perceived: but facts may be adduced however, that prove its existence. If a little vessel of iron or platina, filled with mercury, be placed in contact with the negative pole, on touching the surface of this fluid metal with the positive conductor, we obtain a point or circle of a powder very different from the black oxide of mercury, which is produced when the mercury is placed in contact with the positive pole, and touched with the negative conductor. The oxide produced in the latter case arranges itself in the figure of little stars, equal to those produced by positive electricity with the powder: and the circular figures on the mercury at the negative pole are likewise equal to those produced by the powder electrified with the negative conductor.

In a pile the poles of which are not made to communicate by means of a conducting substance, the chemical action of the strata composing it is very unequal. The plates of zinc are oxided less in proportion to their distance from the positive pole; so that those nearest the negative pole have frequently no traces of oxidation, and seem rather to have been protected from the action of the water by which they are wetted, than attacked by the action of the pile. This may be rendered still more evident by placing every fifth pair in contact with an iron wire, the other end of which is plunged in water. In this experiment the oxidation of these wires will be in the inverse ratio of their distances from the positive pole; in the centre the wire will not be more oxided than another simply plunged in water for an equal length of time, and all the wires beyond this will be still less oxided. Hence it is evident, that another action, the reverse of oxidation, has taken place.

Of all the effects of the pile, its action on the human body has been least examined. The shock, or rather palpitation, that it excites, has been considered as too simple, to be subjected to strict inquiry; and the flash, as well as its action on the

combine with the hidrogen of water; the positive with its oxigen.

The hidrogenation of metals has different degrees, as well as their oxigenation.

Silver highly hidrogenated becomes a gas: less so, remains solid.

Galvanism hidrogenates and oxigenates metals in the dry way also.

If the positive pole of the pile be armed with gold leaf, and the negative with a bit of charcoal, on making the contact, the gold leaf will burn with a bright flame, and the charcoal remain unaltered: if the situation of the two be reversed, the charcoal will burn, and the gold melt.

The hidrogenation is less distinct: but if mercury, in a vessel of iron or platina, be placed in contact with the negative pole, and its surface touched by a conductor from the positive, a spot or circle of a powder different from the black oxide, which is produced under the opposite circumstances, will be found on the surface.

the tongue, has drawn but slight attention. It is true these inquiries, like all others relating to organized beings, are very difficult, particularly when they concern an action that is frequently injurious to the living subject. Ritter has more than once paid for the following discoveries, by long and even dangerous fits of illness.

It is well known, that the skin, being a bad conductor, must be wetted, to make it a good one: it is likewise found in practice, that a surface of considerable extent must be wetted and armed with metal, to have all the possible effect of a pile. The reason is not difficult to discover, though it may lead to many important consequences: we have only to advert to the known fact, that conductors can only convey a quantity of electricity proportionate to their surface; whence it follows, that, to produce the greatest effect, a considerable extent of skin must be made a good conductor. If one of the surfaces wetted and armed with metal, which is touched by the conductors of the pile, be larger than the other, the sensation is less distinct than that which takes place on the smaller, where there is a more perceptible, and often painful sensation: so that we are masters of the magnitude of the effect that we would produce on any part of the body, a very important circumstance in employing galvanism medicinally.

The following is an application of what has just been said. All the difference between the shock obtained from the pile, and that received from a jar, arises from the different states in which we are when we touch them: if a very large pile be touched with dry hands, we experience the same sensation, as if we had touched a charged jar: on the contrary, if with hands thoroughly wetted and armed with metal we touch a jar previously discharged by dry hands, we receive a shock similar to that of the galvanic pile.

Ritter reduces all the effects of the pile on the animal body to expansions and contractions. By the positive conductor he has made several parts of the human body assume a greater bulk; and by the negative he has made the same parts contract. When the tongue is brought into contact with the positive conductor, applying the negative to some other part of the body, and they be all left a few minutes in this state, a little rising is produced on the tongue: the negative conductor, placed in contact with it in the same manner, produces a little depression.

In the latter case the black oxide arranges itself in stars, as when electrified by the machine positively; and the circular figures of the former resemble those produced by electrifying the powder negatively.

When the poles are not made to communicate by means of a conductor, the chemical action or the strata is very unequal. The plates of zinc are oxided less in proportion to their distance from the positive pole; and those nearer the negative pole appear to have been protected from this action.

If every fifth pair be in contact with an iron wire communicating with water, the oxidation of the wires will be in the inverse ratio of their distance from the positive pole; the central wire will not be more oxided, than one simply standing in water, and the wires toward the negative pole will be preserved freer from oxidation.

3. & 4. Spark and shock.

The action of the pile on the human body has been least examined.

It is in fact most difficult, and Ritter has brought on himself long and dangerous fits of illness by his experiments. The skin, to form a good conductor, must be wetted; and this to a considerable extent, to have all the effect of a pile. If the skin be wetted and armed with metal in two places, and touched with conductors from the pile, the sensation on the smaller surface is most perceptible, and often painful: hence we can command the magnitude of the effect we would produce on the body. The difference between the shock from the pile, and that from the jar, arises from the state in which we are when we touch them. If we touch a large pile with dry hands, we receive a smart shock; but if with hands wetted and armed with metal, we touch a jar discharged previously by dry hands, we feel the same sensation as from the pile.

depression. If the wet hands be kept a few minutes in contact with the poles of the pile, the pulse of the hand in contact with the positive pole becomes sensibly stronger, and that of the hand which touches the negative pole weaker. Mr. Ritter has given us many particulars on this subject, and is employed in making additions to them; but we shall content ourselves with observing, that the expansion is followed by a sensation of heat, the contraction by a sensation of cold.

The action of the pile on the organs of sense is modified by the particular nature of each; but it is remarkable, that the two poles of the pile produce in some sort the two extremes of each species of sensation. I have already observed, in the abstract I gave of Mr. Ritter's discoveries on light some time ago, that the pile produces in the eyes those red and blue colours, which are nearly the extremes of those of the prism; and if it were not so difficult to distinguish the violet from the blue, undoubtedly we should have nothing to wish in this respect. In these experiments, the eye in the positive state, while it sees every object of a red colour, sees them at the same time larger and more distinctly: in the negative state, on the contrary, it sees them at once blue, smaller, and less distinct than they usually appear.

Thus the expansive power of the positive pole, and the contracting power of the negative, seem to exert their action here likewise.

The tongue is equally affected by the pile: the acid taste produced by the positive conductor, and the alkaline by the negative are sufficiently known.

The effect of the negative conductor on the nose is an ammoniacal smell; that of the positive is a depression of the sensibility of the organ, similar to what is produced by the oxigenated muriatic acid.

The ears, touched by the positive conductor, hear a grave sound, and with the negative, a sound more acute.

These experiments require much care: to repeat them properly it is necessary to read the descriptions at large, which the author has given in different tracts, where the particulars are minutely detailed.

Ritter reduces all the effect of the pile on the body to expansion and contraction; the positive conductor expanding, the negative contracting the part. The pulse is made stronger by the positive conductor; weaker by the negative. The expansion is followed by a sensation of heat, and *vice versa*. The action of the pile on the organs of sense depends on the nature of the organ; but the two poles produce the extremes of the sensation. The eye in the positive state sees objects red, large, and distinct; in the negative, blue, small, and more obscure. On the tongue the positive pole produces an acid taste; the negative, an alkaline. In the nose the negative produces an alkaline smell; the positive, that of oxigenated muriatic acid. In the ears the positive produces a grave sound; the negative, an acute.

I X.

Account of a Machine for laying Land level.
By Mr. David Charles *.

THIS fimple machine, which is the invention of my Steward, and of which I have feen nothing fimilar, appears to me neceffary, even in the moft fertile parts of England, where the new fyftem of drill-hufbandry has been introduced, or even where there is any attention to the wafte of time, or to the eafe of cattle in the act of ploughing; in order to get rid of crooked or unequal ridges, without either a fummer fallow by crofs ploughing, or elfe by frequent repetitions of ploughing in the winter and fpring, which the humidity of our climate will not allow in every kind of foil. Account of a machine for laying land level.

I reduced fourteen acres of land laft fpring to a perfect level, where the crowns of the ridges were above two feet higher than the furrows, and where they were crooked and of unequal breadths. Six acres of this is now under turnips, a crop that gives fufficient time to ameliorate the under-ftrata of foil that had perhaps never before been expofed to the influence of the fun and air; and by the adoption of the Northumberland mode of fowing that root on dunged drills, it is almoft immaterial where the upper ftrata is, provided the feed vegetates, as it foon ftrikes into the manure, and rapidly flourifhes.

My chief fuccefs, however, has been upon a field of eight acres, which lay in the unprofitable ftate already defcribed. This land, which is a deep clay, and which had produced a crop of wheat from an old lay-fod the former year without any manure, was winter ploughed, and lay in that ftate until the leveller was introduced the firft dry weather in April. It was preceded by two horfe-ploughs, taking perhaps a fquare of an acre at once: thefe loofened the foil the depth of a common furrow, and twice the breadth acrofs the ridges. The leveller followed, drawn by two oxen and two horfes, with a man at each handle, to prefs it down where the weight is to be removed, and to lift up the body by the handles

* From the Tranfactions of the Society of Arts, who rewarded the Inventor with their filver medal. The communication was made by Lieut. Col. Hardy, of Weftmead, Carmarthenfhire.

where

Account of a machine for laying land level.

where it is to be difcharged. Thus four men, one driver, and eight head of cattle, will more effectually level from half an acre to three roods in one day, according as the earth is light or heavy, than fixty or eighty men would accomplifh with barrows and fhovels, &c. even with the affiftance of a plough. In fandy ground where the depth of one furrow will bring all to a level, as much will of courfe be done in one day as two ploughs can cover; but my ground required to be gone over feveral times. After this field was levelled, the backs of the ridges, as they are termed, which were ftripped of their vegetable mold, were ploughed up, the furrows not requiring it. They were alfo harrowed, and the field copioufly manured with lime compoft; harrowed in, and broke into nine-feet ridges, perfectly ftrait, in order to introduce Duckit's drill. It was fown under furrow, broadcaft, the laft of it not until the 13th of May, and was cut down a reafonable crop the 4th of September. I am now thrafhing it, and a fample fhall be fent, as well as a return of the eight acres if neceffary.

The field now lies in proper form, well manured, with the advantage of a fair crop from heavy tenacious ground, without lofing a feafon, and in a year by no means favourable.

I am well aware there are many fhallow foils, where it may be hazardous to remove the enriched furface, and truft perhaps one half of your land for a crop that had never before been expofed to the atmofphere; but where the foil is fufficiently deep, or you have good under-ftrata, and there is manure at hand to correct what is four from want of expofure and tillage, it is evident from this experiment that no rifk is run.

To avoid the expence of a fallow, and to lay out ground in ftrait and even ridges, even where drill hufbandry is not practifed, fhould be objects to every rational farmer. But where the new fyftem is intended to be adopted, it becomes indifpenfably neceffary. In laying down lawns, parks, &c. where furrows are an eyefore, or places inacceffible to wheel carriages from their declivity, and from which earth is to be removed, it will be found equally ufeful.

Should the fociety confider the inventor, David Charles, worthy of any renumeration, honorary or otherwife, it will be gratefully acknowledged by

Your obedient Servant,

Weftmead, *Jan.* 1, 1803. JOSEPH HARDY.

4

Certificates

Certificates from Mr. Owen Edwards, of Brook, and Thomas Bynan, carpenter, of Weſtmead, accompanied the above letter, confirming the ſtatement made herein.

Account of a machine for laying land level.

Deſcription of the Machine for laying Uneven Land level, invented by Mr. DAVID CHARLES.—*Plate* XI. Fig. 1, 2.

Fig. 1.—A, Part of the pole to which the oxen or horſes which draw the machine are faſtened, and which is attached to the machine by a pin at B.

CC, The two wheels, ſhod with iron, which run upon the axle D.

EE, The upper frame-work of the machine, extending from the axle to the extremity of the handles FF, and ſecured firmly by the croſs pieces.

GG, The curved iron ſliders of the machine, which may be raiſed or depreſſed a little by means of the pins HH, which paſs through holes in the wood-work, and alſo in the iron ſliders; theſe ſliders form one piece with the back iron ſcraper I, in the manner more fully explained in *Fig.* 2.

K, The wooden back of the machine, which ſhould be made ſtrong, to reſiſt the weight of the earth when collected therein. The iron ſcraper ſhould be firmly ſecured to this by ſcrews and iron-work.

LL, The wooden ſides of the machine firmly connected with the back and frame-work, in order to aſſiſt in collecting the earth to be removed.

M, A ſtrong croſs piece into which the ribs which ſupport the back are well morticed.

Fig. 2.—K, The interior part of the back of the machine.

I, The iron ſcraper, ſharp at the bottom, and firmly ſcrewed to the back of the machine.

GG, Parts of the ſide irons or ſliders, ſhowing the mode in which they are united with the ſcraper I.

M, The croſs piece above deſcribed.

Experiments

X.

*Experiments on Magnetism; by Mr. Ritter, of Jena. Communicated by Dr. Orsted, of Copenhagen**.

The phenomena of magnetism and electricity have often been compared, but we want facts.

THE phenomena of magnetism have frequently been compared with those of electricity, and many facts seem to justify the comparison. These facts, however, are neither numerous enough, nor sufficiently conclusive, to compose a complete theory. A series of experiments, exhibiting the magnetic needle in all its relations to electricity, at present better known by means of the pile, would undoubtedly throw much light on a subject heretofore so obscure. Ritter felt the importance of such an undertaking, and began fresh inquiries concerning magnetism, with the same ardour and sagacity that have ever distinguished his labours. Though these experiments did not always answer the full extent of his designs, they notwithstanding exhibit a sufficient number of interesting facts to excite the curiosity of every natural philosopher.

Ritter has instituted new inquiries on the subject.

His experiments did not always answer, but exhibited many interesting facts.

A magnetic wire, and another not magnetic, excited galvanic palpitations in frogs; the south pole more strongly, the north less, than iron not magnetic.

Mr. Ritter's first experiments with the magnet were on frogs. He found that a magnetic iron wire, with another not magnetic, excited a galvanic palpitation in these animals. Presently he observed, that the south pole excited stronger palpitations, and the north pole weaker, than the iron not magnetic. Having constantly noticed, that the metals most susceptible of oxidation excited the strongest palpitations, he inferred, that the south pole possesses a greater affinity for oxigen than simple iron, and the north pole less.

Hence he inferred, that the south pole has a greater affinity for oxigen; the north, less.

This confirmed by experiment.

This supposition he confirmed by means of several chemical re-agents. He placed a magnetic iron wire on pieces of glass in a plate of earthen ware, and poured upon it very weak nitric acid. The south pole was attacked by the acid much more powerfully than the north; and was soon surrounded by a deposition of oxigen, the quantity of which greatly exceeded that of the other pole.

Magnetic iron wires being placed in weak nitric acid, the south pole was most oxided.

The south pole of a magnetic wire being immersed in one phial of water,

The different oxidability of the magnetic poles is very well exhibited likewise, by taking three small bottles of equal size, filled with water, either pure or slightly acidulated, and putting

* Journal de Physique, December, 1803, Vol. LVII. p. 406.

into

into one the ſouth polar end of a magnetic wire, into a ſecond the north polar end of a ſimilar wire, and into the third the end of an equal wire not magnetic: the ſouth pole will firſt begin to depoſit oxide, the unmagnetic iron a little after, and the north pole laſt. This experiment requires conſiderable care. The ſurface of the water muſt be covered with very freſh oil of almonds, to exclude all acceſs of air. Care muſt be taken too, that one of the bottles is not more expoſed to the ſun than the others, becauſe light accelerates oxidation. Ritter convinced himſelf of this by direct experiments; expoſing two iron wires in water to the ſun, but covering one of the phial with black paper, when that in the phial left uncovered was oxided much the more quickly.

the north pole of one in another, and the end of a ſimple wire in a third; the ſouth pole firſt depoſits oxide, and the north pole laſt. This experiment requires particular precautions. Air muſt be excluded; and light accelerates oxidation.

If infuſion of litmus be ſubſtituted inſtead of the water in the three phials in the preceding experiment, the relative oxidations will be the ſame; but they will be attended with a change of colour, ſhewing, that an acid is produced proportional to each oxidation; ſo that the ſouth pole not only undergoes the greateſt oxidation, but likewiſe reddens the infuſion of litmus moſt *. The action that takes place in this experiment is very feeble, and frequently requires a week or more to produce a diſtinct effect; and indeed to accelerate it ſo much as this, it is neceſſary to add, previouſly to the infuſion, as much acetic acid as will incline it to red, without completely changing its colour. The infuſion reddened in this experiment reſumes its blue colour on expoſure to the air: but we muſt not hence conclude, that the acid produced by the action of the magnet is very volatile, for infuſion of litmus reddened by phoſphoric acid, or any other, exhibits the ſame phenomenon.

If infuſion of litmus be uſed, the relative oxidations will be the ſame; but an acid in proportion to each will be produced, as appears by the change of colour. This action is very ſlow; and to accelerate it a ſmall portion of acetic acid ſhould be added to the infuſion. The infuſion thus reddened becomes blue again on expoſure to the air: but it is the ſame when reddened with any other acid.

The following experiment exhibits ſome things peculiar, and therefore I ſhall give it more at large. It has not been repeated, but the harmony of its reſults is in favour of its accuracy. Sixteen magnetic wires, of equal ſize and power, were placed in ſix veſſels, all equally full of a mixture of one part nitric acid and thirty-ſix parts water, in the following

Experiment.

* Ritter has remarked, that the oxidation of zinc, and ſeveral other metals, in pure water, produces an acid. Mr. Jager, a celebrated phyſician of Stuttgard, made the ſame diſcovery, without knowing any thing of Ritter's.

Ritter and Jager have found, that the oxidation of ſeveral metals in pure water produces an acid.

manner:

In phials containing water 36 parts, nitric acid 1 part, were immersed: 1. a N. and S. pole, at $\frac{1}{2}$ a line distance: 2. the same $1\frac{3}{4}$ inch: 3. 3 S. poles, at $\frac{1}{2}$ a line: 4. the same at $1\frac{3}{4}$ inch: 5. 3 N. poles, at $\frac{1}{2}$ a line: 6 the same at $1\frac{3}{4}$. The oxide deposited in these with respect to quantity was in the following order: No. 2, 1, 3, 4, 5, 6. The loss of fluid by evaporation, was in the inverse ratio of the oxidations. All the magnets were weakened in power: No. 2 least; 1 more: in all the rest one wire was weakened more than the others. On immersing two south poles in one phial, two north in another: the latter were most oxided. Mr. R. attempted unsuccessfully to construct a battery with 120 magnetic wires, their poles placed in opposition, and separated by a drop of water; but he has not given up the design: and means to pursue his inquiries on the subject at large very extensively.

manner: In the first glass were placed two wires, one with the north pole immersed in the fluid, the other with the south, and not more than half a line asunder: In the second, the same, but the wires an inch and three quarters apart: In the third and fourth were each three wires, with the south poles of all immersed, but their distances in the two glasses different, as in the first and second: In the fifth and sixth were wires similarly arranged, but with the north poles immersed. Different quantities of oxide were gradually deposed; and to express the whole in few words, we will call the south pole S, the north pole N, their greater distance g, and their less p; and we will express the order of oxidations as follows: $S N g > S N p > 3 S p > 3 S g > 3 N p > 3 N g >$. On the nineteenth day it was observed, that the loss of fluid by evaporation had not been equal in all the vessels, but took place in the inverse order of the oxidations. All the magnetic wires were weakened in power; $N S g$ least; $N S p$ more: of the wires $3 S p$, two had lost less power than the third; and in like manner $3 S g$, $3 N p$, $3 N g$, had each two left more powerful than the third; the strongest were equal to $N S g$.

In another experiment, where two little vessels filled with infusion of litmus were employed, one of them containing two magnetic wires, the south poles of which were immersed in the fluid; the other two similar wires, of which the opposite poles were immersed; the oxidation was greatest in the latter vessel*.

Lastly, Mr. Ritter endeavoured to construct a battery of magnets, but he did not succeed. For this purpose he employed a hundred and twenty magnetic wires, placed so that each pole had its contrary opposite to it, and separated from it by a drop of water; but this apparatus produced no effect. The ingenious author, however, has not relinquished the hope of being able to compose a magnetic battery, though other experiments, not less important, have hitherto prevented him. This series of experiments he considers only as the commencement of a very extensive labour, the results of which we hope soon to obtain.

* This appears contradictory to the experiment adduced in the third paragraph. T.

Observations

XI.

Obſervations on Arſeniated Copper. By HAUY *.

THE only ores of arſeniated copper which are well known, are thoſe from the county of Cornwall in England. The determination of their true compoſition followed cloſely on the diſcovery of this metallic ſubſtance, for which we are indebted to the fortunate chance which threw ſome ſpecimens into the hands of the celebrated Klaproth. It was in 1787 that he publiſhed in the *Journal de la Société des Curieux de la Nature* †, the reſult of the examination which he had made of this new mineral. History of the diſcovery of the ores, and their compoſition.

The authors who have ſpoken of arſeniated copper ſince that period, had only deſcribed it under the form of acicular cryſtals, when Citizen Lelievre, member of the Council of Mines, having ſuſpected the exiſtence of a peculiar ſubſtance, from the inſpection of a group of green hexagonal bevilled laminæ which were given to him, made an eſſay of it, and diſcovered the preſence of oxide of copper and arſenic acid. Citizen Vauquelin ſoon afterwards confirmed this indication, and determined the proportion of the relative quantities of the two principles contained in the ſame ſubſtance.

About this time the opening of a ſecond mine in the county of Cornwall occaſioned the re-appearance of arſeniated copper, the vein which had been formerly explored being exhauſted. This diſcovery was the more important, as the ſubſtance appeared, in its new ſituation, with characters altogether peculiar, and under forms hitherto unknown.

M. de Bournon, who was at hand to participate in this increaſe of riches which reſulted to mineralogy, ſent to Citizen Gillet Laumont and me, ſeveral ſpecimens choſen from among thoſe he poſſeſſed; and that which added to the value of his gift was his haſte to communicate to us the intereſting work which he had prepared, on the cryſtallography of arſeniated copper, before he publiſhed it.

* Tranſlated from a pamphlet in quarto, ſent by the author to the Count de Bournon; probably forming part of a journal.

† Tom. VIII. p. 160.

Mr. Chenevix was employed at the ſame time on the analyſis of this ſubſtance. Soon after they both publiſhed the reſults of their reſearches in the *Philoſophical Tranſactions* *; and Mr. Chenevix teſtifies his admiration at ſuch a perfect agreement between two ſciences which employ two methods ſo different to interrogate nature. M. de Bournon, on his part, ſays that the analyſes of Mr. Chenevix have given the moſt ſatisfactory ſanction to the diviſion which he had himſelf made, of the arſeniated copper into four diſtinct ſpecies.

Deſcription of the varieties.

Before going farther, it is neceſſary to make known the varieties of arſeniated copper which I have been enabled to examine. I ſhall confine myſelf to giving a deſcription of them, ſuccinct and independent of the laws to which the ſtructure of cryſtals is ſubmitted, the actual ſtate of our knowledge on this ſubject only admitting of hypothetical views, of which I ſhall ſpeak hereafter.

1. Obtuſe octahedral arſeniated copper, (*Plate* IX. *Fig*. 2): incidence of P on p, 50° 4′; of P' on p', 65° 8′; of P on P', 139° 47′ †. The colour of the cryſtals is ſometimes a fine celeſtial blue, and ſometimes a green, which varies between a graſs-green and a pale green. The octahedron ſometimes becomes cuneiform, lengthening ſo that the terminal edge is parallel at D.

2. Lamelliform arſeniated copper. In hexagonal laminæ, whoſe narrow faces are inclined alternately in contrary directions; the incidence of two of the narrow faces, ſituated on the ſame ſide, on the correſpondent baſe, 135° nearly, according to M. de Bournon: incidence of the third on the ſame baſe, 115° nearly.

* For 1801, p. 199, *et ſeq:*

† I adopt here, very nearly, the reſults of M. de Bournon, who indicates 50° for the incidence of P on p, and 65° for that of P' on p'. I have only endeavoured to find limits capable of facilitating the calculations which I propoſe to make. Let bac, gac, (*Fig*. 4) be the ſame faces as P and P', (*Fig*. 2); let ao (*Fig*. 4) be the height of the pyramid which has its ſummit in A, (*Fig*. 2); on (*Fig*. 4) a perpendicular to bc, and or perpendicular to cg:—if $ao = \sqrt{588}$, $on = \sqrt{2695}$, and or $\sqrt{1440}$, we ſhall have 50° 4′ for the incidence of P on p (*Fig*. 2), and 65° 8′ for that of P' on p': whence is deduced, by calculation, 139° 47′ for that of P on P'.

The

The laminæ divide parallel to the large faces with great eafe. Their colour is a fine grafs-green.

3. Acute octahedral arfeniated copper, (*Fig.* 3): Incidence of r on r', 96°, according to M. de Bournon; of l on l', 112°. The colour is a brown green more or lefs deep.

a, cuneiform. The preceding octahædron lengthened fo that the terminal edge is parallel to *n*. This form, which is the moft common, offers the appearance of a long rhomboidal prifm, more or lefs acute, and is terminated by dihedral fummits.

4. Trihedral arfeniated copper. In a ftrait triangular prifm, which is at the fame time equilateral, according to M. de Bournon.

When the cryftals have not been long expofed, their colour is a fine bluifh green; but their furface is fubject to change and take a blackifh tinge. If they are fcratched, their primitive colour will re-appear.

5. Capillary arfeniated copper. This is properly the olivenerz of the German mineralogifts.

6. Mammellated arfeniated copper. In mammellated maffes, ftriated in the interior. Thefe two laft varieties are fufceptible of a great diverfity of tints, which fhew the tranfitions from grafs-green to olive-green, to greenifh-brown, to tawny (*mordoré*), to yellow, to bluifh, and to white, which is frequently fattiny.

The following is the manner in which M. de Bournon claffed the different modifications which have been juft mentioned, according to the differences which they offer with refpect to form, fpecific gravity and hardnefs. M. de Bournon's claffification.

He divides them, as I have faid, into four diftinct fpecies. The firft is derived from the obtufe octahedron: the type of the fecond is the lamelliform cryftal, in hexagonal laminæ, with bevils inclined alternately in contrary directions. He takes the acute octahedron for the primitive form of the third, and connects the acicular cryftals and mammelary concretions with it, as varieties: in the fourth he places the equilateral triangular prifm, and feveral other forms which offer the fame prifm, truncated on its folid angles or on its edges. Four diftinct fpecies.

On the other hand, Mr. Chenevix has given fix refults of the analyfis of arfeniated copper, which I fhall detail, difpofing them conformably to the order eftablifhed by M. de Bournon. Mr. Chenevix's analyfes.

First

First Species, in obtuse octahedra.

Oxide of copper - - -	49
Arsenic acid - - - -	14
Water - - - -	35
Loss - - - -	2
	100

Second Species, in lamelliform Crystals.

Oxide of copper - - -	58
Arsenic acid - - - -	21
Water - - - -	21
	100

Third Species, in acute octahedra.

Oxide of copper - - -	60
Arsenic acid - - - -	39.7
Loss - - - -	0.3
	100.0

Variety of the same Species, in capillary Crystals.

Oxide of copper - - -	51
Arsenic acid - - - -	29
Water - - - -	18
Loss - - - -	2
	100

Another Variety, in mammellated Concretions.

Oxide of copper - - -	50
Arsenic acid - - - -	29
Water - - - -	21
	100

Fourth Species, in trihedral Prisms.

Oxide of copper - - -	54
Arsenic acid - - - -	30
Water - - - -	16
	100

Towards

Towards the conclusion of his memoir, Mr. Chenevix remarks that the natural arseniate of copper exists in three different combinations, the first of which contains 11 *per cent.* of arsenic acid, (first result above); the second contains 21, (second result), and the third [illegible], (third, fourth, fifth, and sixth results. It is true, the third result gave 39.7 of acid in the 100 parts, but as the remainder of the mass was composed of 60 parts of copper without water, he found that the proportion of the acid with the copper did not differ much from that which takes place in the varieties in which water forms a part. this induced Mr. Chenevix to comprehend this result in the same division. Yet he considers this combination as the only true arseniate of copper, while the other three are arseniates of hidrate of copper.

Only one true arseniate of copper, the others are arseniates of hidrate of copper.

I feel the value of the double work from which I have given this extract so much the more, because, having read the memoirs which contain the developement, I am enabled to judge of the advancement which it has produced in our knowledge on a subject which was in a great measure new when MM. de Bournon and Chenevix began to be employed with it. The exposition which I shall add of some enquiries I have made on the crystallization of arseniated copper, and of the reflections which they have given rise to, have no other object but that nothing may be neglected which tends to elucidate in a greater degree every thing connected with an object of such importance as the distinction of mineralogical species.

After having read the crystallographic part of the work in question, I was desirous to know if it was not possible to bring some of the crystals, described by M. de Bournon as belonging to different species, to the same form of the integrant molecule; but not being able to make all the direct observations which would have guided me in this enquiry, I was obliged to confine myself to simple hypotheses.

Can the varieties be reduced to one primitive form?

I therefore considered the obtuse octahedron as performing the functions of the primitive form; and I was the more warranted in conceiving this opinion, because the celebrated Karsten, in a supplement to the excellent memoir which he had published before *, on the combinations of copper with different principles, says that the octahedron in question is lamellated,

* *Journ. de Physique*, Brumaire, An. X, p. 345, et suiv.

in

Comparifon of the acute and obtufe oćtahedra.

in a direćtion parallel to the faces of the two pyramids, of which it is the aggregate *. Proceeding on this datum, I was curious to know if it was not poffible to conneć with the form of the obtufe oćtahedron in queftion, that of the acute oćtahedron, which M. de Bournon has taken for the type of his third fpecies. Let P, P' (*Fig.* 2) be ftill an obtufe oćtahedron, in which the incidence of P on p is reckoned to be 50° 4′, and that of P' on p', 65° 8′, conformably to the meafures indicated above: if we imagine another oćtahedron (*Fig.* 3) the fign of which is $\begin{matrix} 2\;4 \\ \mathrm{D}\;\mathrm{F} \\ 2\;4 \\ l\;\;r \end{matrix}$, we fhall find that the incidence of l on l' is 109°, and that of r on r' is 93° 36′. Now the correfpondent incidences determined by M. de Bournon, are one 112°, and the other 96°; which in one cafe makes a difference of 3°, and in the other of 2° 24′.

The differences in the angles accounted for.

If the meafures had been taken on cryftals fo well defined, that the differences could be confidered as real, we muft have concluded that they formed two diftinćt fpecies, becaufe even thefe differences could only have been done away by fuppofing the laws of decrement to be much too complicated to be admiffible.

But if the cryftals were not capable of very accurate meafurement, we can the better conceive that the differences were fimply apparent, and that it may be poffible that the error did not wholly arife in one obfervation, fince it was neceffary to make two, in which fmall deviations might have been produced on oppofite fides; and then mechanical divifion alone, by giving different refults with refpećt to the two oćtahedra, would have fhown that a conformity between the angles obferved and thofe meafured, would be purely accidental.

Comparifon of the lamelliform variety with the obtufe oćtahedron.

I afterwards compared the lamelliform variety with alternate bevils, which is the fecond fpecies of M. de Bournon, with the fame oćtahedron with obtufe fummits. Now, if we fuppofe two interfećting planes, parallel to the face P′, and which meet the center, they will detach an oćtahedral fegment which cannot be fuppofed to have much thicknefs, and whofe two large faces will be hexagons, and the fix lateral faces, trapeziums,

* Journ. de Phyfique, Pluviofe, An. X. p. 151.

inclined

inclined to the great faces *. But these trapeziums will not be situated alternately in opposite directions. The three which will form obtuse angles with the large faces, will be contiguous to each other, and the same will be the case with those which form acute angles with the same faces. For example, those of the trapeziums which form obtuse angles with the large face analogous to *P'*, will correspond to the two faces of the adjacent octahedra at *B*, *B'*, and to the face situated behind *A*, parallel to *P*. The inclination of this latter face on *P* is, according to M. de Bournon, 115°; and the two others, as I have indicated them above according to my calculations, are each about 139½°.

Now, of the three lateral trapeziums in the lamelliform arsenical copper, one has the same inclination of 115° on this base, according to M. de Bournon, and the two others have 135°; an estimate which he only gives as an approximation, and which only differs by 4½° from that corresponding to it in the obtuse octahedron, (*Fig.* 2).

The great difference consists in this, that the three lateral trapeziums which look towards the same base, in the octahedral segment I have described, are contiguous to each other, as I have said; while those of the lamelliform arseniated copper alternate with the three others which look towards the opposite base †.

But there is a method of removing this difficulty. Let us conceive that the two sections made in the octahedron (*Fig.* 2), instead of being parallel to the face *P'*, are so to the face *P*. In this case the lateral trapeziums, situated on the two sides of the edges *B*, *B'*, will always have an inclination of 139½° to the superior base. Now, if the segment parallel to *p* makes an angle of 115° with the base analogous to *P*, the three segments will preserve, with respect to those turned towards the opposite base, the alternation indicated by M. de Bournon.

* Several substances, among others the spinelle, offer examples of similar segments.

† The figure given by M. de Bournon, of which that is a copy in *Plate XI. Fig.* 5, seems to have been traced according to the condition that the three trapeziums turned towards the same base should be contiguous. This was doubtless an oversight of the draughtsman.

But the incidence of p on P gives, on the contrary, an acute angle of 50°. Now, let us imagine a decrement indicated by $_{,}D$, which acts on the face p and on that which is oppofite; the faces produced will be fituated vertically; whence it follows that that which will mafk the face p will form an angle with P equal to 90° *plus* 25°, which is the half of the inclination of p to P, that is to fay, the angle in queftion will be 115°, conformably to obfervation *.

Their divifion into four fpecies is not admitted by the laws of ftructure.

I fhall not urge thefe refults farther, which, as I have already ftated, I only offer as purely hypothetical; and I fhall abftain from adding my opinion with refpect to the fourth of the fpecies admitted by M. de Bournon, which, according to him, has the equilateral triangular prifm for a primitive form. It is enough for me to have fhown that, with refpect to the divifion of arfeniated copper into four diftinct fpecies, the laws of the ftructure may give rife to doubts which deferve fome attention. If they can be removed, as it is not impoffible they may, another proof will be obtained in favour of an opinion on which no obfcurity fhould remain, that it may be worthy of being unanimoufly adopted.

Comparifon of Mr. Chenevix's analyfes and M. de Bournon's divifion.

If we now confider the refults of the analyfes which Mr. Chenevix has made of the different modifications of arfeniated copper, we find, that in fuch of thefe analyfes as have had for their object the types of the four fpecies admitted by M. de Bournon, three have given fenfible differences in the relative quantities of copper, arfenic acid, and water. Thefe analyfes correfpond with the firft, third, and fourth fpecies. Another analyfis, made on the fecond fpecies, gave only copper and arfenic acid, without water. Thus, fuppofing that the relations between the quantities of the three principles contained in the modifications which Mr. Chenevix calls *arfeniates of hidrate of copper*, conftitute true limits, and that, in the modification which he calls fimply *arfeniate of copper*, the abfence of water depends on the nature of the fubftance itfelf; we fhall, in this refpect, find an agreement between the refults of

* I have a lamelliform cryftal, on which, inftead of a fimple bevil, there are two, fituated in contrary directions on the fides of the fame edge; but they are too fmall for it to be poffible to determine the pofitions exactly.

analyfis and thofe of cryftallography, very favourable to the fubdivifion of the mineral in queftion, into four diftinct fpecies.

But Cit. Vauquelin, on analyfing a piece of lamelliform arfeniated copper, whofe cryftals were quite frefh, obtained a very different relation between the quantities of the three principles*. His refult was as follows: Vauquelin's analyfis of the lamelliform arfeniate:

Oxide of copper	39
Arfenic acid	43
Water	17
Lofs	1
	100

It is remarkable in this cafe, that the quantity of acid exceeds that of the copper, while in the refult obtained by Mr. Chenevix it forms only a little more than a third of the quantity of copper. It is not therefore evident that the limits indicated by this celebrated chemift are effential to the fubftances analyfed.

The experiments of the fame philofophers on the capillary cryftals, offer differences not lefs ftriking. According to Cit. Vauquelin thefe cryftals contain, and of the capillary cryftals.

* Mr. Chenevix, in his memoir, gives a paffage from a letter which Citizen Vauquelin had written to him, and in which he informed him, that having analyzed cryftals of the lamelliform variety, he found that they were compofed of 59 of oxide of copper and 41 of arfenic acid. Mr. Chenevix adds, that the great difference between this refult and that which he had himfelf obtained from the fame fubftance, induced him to repeat his analyfis with great care and attention, and that he conftantly found the fame proportions of oxide of copper, arfenic, and water. It is very probable that this refult announced by Cit. Vauquelin, and fo different on the other hand from that now given, was obtained in a firft effay, or that this chemift, when he wrote to Mr. Chenevix, trufted to his memory, which was not fo faithful as it generally is. However that may have been, the only refult avowed by Cit. Vauquelin is that we now publifh, and which he has inferted in the *Journal des Mines*, No. 55, p. 562.

Silex

Silex - - - -	2
Water - - - -	5
Arseniate of iron - - -	7 to 8
Arseniate of copper - - - -	86
	100

This chemist adds, that if the arseniate of copper did not contain any foreign matter, it would be formed of about 69 parts of oxide of copper and 31 of arsenic acid.

Klaproth's analysis.

We have another result on the same subject, obtained by M. Klaproth, whose labours have concurred so advantageously with those of Vauquelin, to procure an exact knowledge of the composition of minerals. His result gave,

Oxide of copper - -	50.62
Arsenic acid - - -	45.00
Water - - - -	3.50
Loss - - - -	88
	100.00 *

The quantity of copper is nearly the same as in Mr. Chenevix's result; but on one side we find 45 of acid with 3.5 of water, and on the other only 29 of acid and 18 of water; which is very different.

Mr. Chenevix's results do not agree with those of M. de Bournon.

Besides, we need only keep to Mr. Chenevix's own results to find difficulties and causes of uncertainty; for while this celebrated chemist obtained a very sensible quantity of water from the capillary crystals and the mammellated masses, these two modifications were considered by M. de Bournon as simple varieties of the third species, which is the acute octahedron, and which gave only copper and arsenic acid without water. Further, if the analyses of the capillary crystals and of the mammellated masses, are compared with that of the crystals in trihedral prisms, it will be seen that the differences do not exceed those which are frequently met with between the analyses of several pieces which evidently belong to the same species of mineral.

I add, that M. de Bournon seems to have had more authority for considering the capillary crystals and the mammel-

* *Mémoires de la Connoissance chimique des Minéraux*, p. 192.

lated

lated concretions as ſimple varieties of the acute octahedron, ſince he indicates the intermediate modifications which connect theſe varieties with their type; ſo that, according to him, there are cryſtals which are perfectly determined in one part of their length, and fibrous at their extremity.

M. de Bournon, doubtleſ ſtruck with the exception which the agreement between the two ſciences, announced by Mr. Chenevix and himſelf, ſeemed liable to, in this inſtance, has ſince inſerted in Mr. Nicholſon's Journal * a note, in which he propoſes to eſtabliſh a fifth ſpecies of arſeniated copper, compoſed of the capillary cryſtals and mammeliated maſſes, which ſeems to operate leſs in removing the difficulty than in bringing it to light. A fifth ſpecies propoſed.

It cannot be denied that the modifications of arſeniated copper offer ſenſible differences in their aſpect, their exterior forms, and their colours. M. de Bournon alſo indicates ſome in their hardneſs and in their ſpecific gravities. But the reduction of natural beings to the ſmalleſt poſſible number of ſpecies, really diſtinct, is an advantage of ſuch value to ſcience, which it perfectionates by ſimplifying it, that, before ſeparating ſubſtances, according to thoſe diverſities which ſeem to be at variance with the relations which they otherwiſe have, and before ſeeking particular ſpecific names for them, which would be neceſſary, all the means of aſcertaining that the diverſities in queſtion are not purely accidental, ſhould be exhauſted. Even though the reſearches which ſtill remain to be made for the accompliſhment of this object, ſhould have no other effect but to cauſe the diſappearance of one ſingle ſpecies, admitted by the two celebrated men whoſe reſults I have ſet forth, from the ſyſtem, they will not be unprofitable to the progreſs of mineralogy. External differences ſhould not be admitted alone as evidence of a different ſpecies.

*** A reply communicated by the Count de Bournon will appear in our next.

* Philoſophical Journal, new Series. Vol. VII. p. 577.

Obſervations

XII.

Obſervations upon the Doctrine of Count Rumford reſpecting the want of direct conducting Power in Fluids with regard to Heat. By CIT. BERTHOLLET.

(*Concluded from page* 140.)

Nicholſon, Thompſon and Murray have proved, that heat paſſes through the particles of fluids; that currents are often imaginary; and that fluids differ in conducting power.

I AM of opinion, that the experiments of Nicholſon, of Thompſon, and of Murray, leave no doubt on the communication of heat between the particles of liquids: ſome of them ſhow that the motions of the ſolid corpuſcules which are agitated in a liquid, may often miſlead, with reſpect to the currents which are believed to be perceptible: but their exiſtence muſt not, for this reaſon, be denied, when a difference between the ſpecific gravities is ſuddenly eſtabliſhed, and when the heat is communicated at the lower part of a veſſel. The others prove that the communication of heat may be made through a liquid in which no current can be ſuppoſed to tranſport it immediately to a ſolid body, and they prove that liquids are poſſeſſed of a conducting faculty which differs in its intenſity; but it is not to be inferred from this, that the locomotion of the particles of liquids does not contribute to eſtabliſh an equilibrium of temperature more ſpeedily: it is even probable, that the latter effect is generally the greateſt.

Theſe general facts account for all the phenomena.

The foregoing conſiderations, into which I have admitted the application of the faculty of communicating heat common to all bodies, of the conducting difference, and of the more ſpeedy diſtribution of heat by means of the difference of the ſpecific gravity which it introduces between the particles of a fluid, ſeem to me to account for all the phenomena which the diſcernment of Rumford has made public.

That gaſes receive heat very rapidly, is ſeen in the expanſion of air thermometers and air balloons.

Theſe conſiderations lead me to an opinion very different from his; it is known with what rapidity the thermoſcopes, or air thermometers, indicate the variations of temperature: Pictet could not obſerve a ſecond of difference between the elevation of a thermometer of this deſcription, and the emanation of radiant heat by a body placed at a diſtance: it has been obſerved, that aeroſtats experience a ſudden dilatation

tion by the appearance of the ſun *; theſe phenomena ſeem to me to indicate that the elaſtic fluids, far from being bad conductors, on the contrary, receive the temperature of other bodies very quickly; for, can it be ſuppoſed that all the particles of the gas take the temperature which they acquire by the contact of the covering of the balloon alone, and how can it be conceived, that the lower particles, which are contiguous to that portion of the covering which does not receive the ſolar emanations, ſhould be carried towards that which is expoſed to it? And ſince theſe particles at each contact only receive a part of the temperature to which they attain, what a prodigious whirlwind muſt there be ſuppoſed to be in the gas!

It appears to me, that the elaſtic fluids, inſtead of being bad conductors, poſſeſs this property in a high degree, although they probably differ from each other in this reſpect; and if air which is confined produces effects which ſeem to prove the contrary, they are owing to ſome circumſtance which modifies this property. The ſlow conducting by confined gaſes aſcribed to ſome modification.

I think it is probable, that this circumſtance is the ſtate of compreſſion produced in a gas which cannot acquire a dilatation ſuitable to the temperature it receives; we have ſeen that caloric, in combining with the gaſes, only raiſes the temperature becauſe the dilatation meets with an obſtacle (107); hence it reſults, that the further the air is removed from the ſtate of dilatation, which it ſhould have, to be in equilibrium of temperature, the greater reſiſtance will it oppoſe to the combination of the caloric, and the more will it loſe of its conducting faculty, ſo that the air which would take the temperature of the ſurrounding bodies with facility, if it could acquire ſuitable dimenſions under a given preſſure, becomes a worſe and worſe conductor in proportion as it receives a temperature farther removed from the dimenſions which it can take. The air then experiences an effect, which may be compared to that of a body in which the force of coheſion obſtructs the action of a liquid, which can effect its ſolution as ſoon as this obſtruction begins to be diminiſhed. The gas is probably prevented from acquiring heat, becauſe it cannot expand in that ſtate.

This explanation is applicable to the conſervative property of heat, which Rumford has proved to belong to the air which adheres to particles, ſuch as thoſe of the eider-down; The effect of eider down, &c. is leſs to prevent the currents, than to oppoſe the ex-

* Deſcrip. de l'aéroſtat de l'Acad. de Dijon.

this

pansion by its attraction, by which the air adheres to it.

this air only adheres by a true affinity, which probably reduces its dimensions, or at least, opposes its dilatation; and if the water can drive it off, it is only because it combines with these substances, and adheres to their surface by its affinity; so that the air will then experience the same effect from the action of the affinity of the bodies to which it is adherent, as is produced on its elastic effort, by the space within which it is confined, and in which it receives a higher temperature without having the power to dilate.

So that elastic fluids being more alterable in their volume, are also more disposed to give and take heat.

Thus the elastic fluids which dilate much more by a similar change of temperature than liquids and solids, must have the corresponding faculty of entering more easily into combination with caloric: they offer but little resistance to compression; they heat by the reduction of their volume; and they cool when they dilate: do not these effects announce a great disposition to combine with caloric, or to abandon it, and to receive different degrees of saturation from it? and nevertheless, according to the opinion of Rumford, there must be an insurmountable barrier between the most distant temperatures, of the different particles of a gas, when the particles do not meet with a solid body.

The same doctrine applied to liquid water: Sudden accumulation or absorption by its fusion, it conducts better than ice, &c.

It is possible, that liquid substances may be much better calculated to conduct heat than when they are in a solid state; the properties of the reciprocal affinity which produces cohesion, seem to point this out: for since this affinity opposes the dilatation, it will offer an obstacle to the combination of the caloric: this resistance to its introduction is also proved by the quick accumulation which is made of it, as soon as the force of cohesion is destroyed, so that it is opposed to the combination of caloric, as well as to that of other substances; in fact, water seems to take the common temperature more easily, independent of the locomotion of its particles, than ice, which is a very bad conductor, and it is perhaps from this difference, that ice, and all the solids which pass to the liquid state, liquefy at the surface, instead of taking the common temperature.

I only offer these last explanations as conjectures, which may invite to experimental enquiries on a subject which is not indifferent to chemical theory.

A Memoir

XIII.

A Memoir on the Movements which certain Fluids receive from the Contact of other Fluids. By* J. DRAPARNAUD, *Curator and Professor of Natural History, at the Medicinal School of Montpelier.*

I HAD observed that alcohol attacks, and finishes by even, at length, destroying the calcareous covering of the mollusceæ, which are put into it to be preserved. Supposing that this might arise from the alcohol, particularly that which is not well rectified, containing a little acetous acid, I put a little tincture of turnsole into a glass capsule, and poured into it a a few drops of alcohol. The tincture did not change colour, but to my great surprise it moved towards the circumference with great vivacity, leaving the bottom of the capsule uncovered: when it had reached the maximum of dispersion, it returned again, and covered the bottom of the vessel which it had abandoned. It is evident, therefore, that missing what I sought, I found that which I did not seek, which frequently occurs in the course of experiments. Experiment on the action of alcohol which led to the discovery of this impulse.

This curious experiment induced me to make a multitude of others, and to try a great number of substances. At the moment of putting these experiments in order, and of composing this memoir, I recollected having read that *M. Benedict Prevost* had produced this repulsion of water by means of volatile oils, and even of many solid odorant bodies. I therefore consulted the two memoirs which this ingenious philosopher has inserted in the Annals, and whose subject is, *The methods of rendering the emanations of odorant bodies sensible to the sight*†. Although my experiments were made with another view, I pass over, in silence, all which are conformable to those of *M. Prevost*, and which I made by employing the same substances: I shall only speak of those which differ from his, either in the results obtained, or in the means employed. Prevost's memoirs on the emanations of odorant bodies.

1. If a thin stratum of water is put on the bottom of a vessel, and a drop of alcohol is brought to the centre of this Action of alcohol on water.

* From the Annales de Chimie. Fructidor, An. XI.

† Annales de Chimie. Tom. XXI. et XXIV.

stratum,

ſtratum, with a glaſs rod, the water flies in an inſtant with vivacity, leaving the bottom of the veſſel uncovered; when it has reached the maximum of diſperſion it returns, and covers again the bottom of the veſſel which it had quitted.

Diſk of diſperſion.

2. I call that part of the veſſel which is abandoned by the water the *diſk of diſperſion.* In the preceding experiment, this diſk ſhows a perfect dryneſs and all its natural poliſh.

The repitition of the experiment leſſens its effects.

3. The repulſion becomes leſs conſiderable, and the diſk of diſperſion ſmaller, in proportion as the experiment is repeated in the ſame water. This ariſes from the water becoming gradually ſaturated with alcohol.

Influence of the veſſels.

4. The nature of the veſſels has no influence on the preceding experiment, nor on thoſe which follow. They take place equally in veſſels of porcelain, earthern ware, glaſs or metal.

5. The form of the veſſels has much influence on the ſecond period of the phenomenon, that is to ſay, on the return of the water, and on the diſappearance of the diſk of diſperſion.

If the veſſel is a little concave, the water always comes back, and covers the bottom of the veſſel again. It will be obvious, that this is the neceſſary effect of its gravity.

If the veſſel is flat, the water only returns when the diſk of diſperſion has not attained too great a diameter.

If the bottom of the veſſel is a little convex, the water does not return after having been diſperſed, and it muſt be evident, that to do ſo it would act contrary to the operation of its own gravity.

Motion of the expelled fluid;

6. Being deſirous to render the obſervation of the motions of the expelled fluid eaſier and more perfect, I ſubſtituted tincture of turnſole, which, as is known, is only water coloured by turnſole, for pure water. The reſults were the ſame, but much more ſenſible; and I could then readily diſtinguiſh an undulating or trembling motion on the internal edge of the water which ſurrounds the diſk of diſperſion; a motion which proves the continual emiſſion of the alcoholic particles againſt this interior edge, and determines the removal of the water.

and of the impelling fluid.

7. But to complete the proof of the explanation which I have juſt given of the phenomenon, it was alſo neceſſary to render the motions of the impelling fluid ſenſible: I ſucceeded by

by an analogous procefs. I coloured alcohol by means of turnfole; with this fubftance it takes a very beautiful colour which is not at all fimilar to the violet blue of tincture of turnfole, but, on the contrary, is of a very vivid blue, analogous to that of indigo, or pruffian blue. I then wetted the bottom of a plate with pure water, and, with a glafs rod, brought a drop of this coloured alcohol to the centre: the water was driven back with vivacity. In the centre of the difk of difperfion was a blue fpot, formed by the coloured alcohol, and the reft of the difk was white like the bottom of the plate. But the proof of the actual continual emiffion of the coloured alcoholic particles, is that as the water retired, its internal edge, which touched the difk of difperfion, became more and more of a violet colour, analogous to that of tincture of turnfole prepared with water. It is evident, therefore that in natural philofophy, facts are explained by facts, and, that this experiment confirms the confequence I had deduced from the preceding experiment.

8. If the plate is wetted with the alcohol, and a drop of water is put into the centre, the alcohol does not experience any motion; the drop of water flattens, it retains its orbicular form for fome moments, at length it finifhes by fpreading irregularly, mixing with the alcohol, and uniting with it. Water does not repel alcohol.

9. If the bottom of the plate is covered with a very thin ftratum of olive oil, and a drop of alcohol is brought to the centre, the oil is repelled, though more flowly than the water on account of its vifcofity, and the bottom of the plate is left dry. Oil is repelled more flowly than water.

If the ftratum of oil is too thick, it will not quit the bottom of the plate, and the expanfive motion of the alcohol takes place only on the fuperfices of the oil.

10. If a morfel of the frefh rind of lemon or orange is placed in the centre of a wetted plate, the water is fenfibly repelled, and the difk of difperfion is agreeably tinged with the prifmatic colours, which depends on the difengagement of the effential oil. But this motion has not nearly fo much intenfity as that produced by means of alcohol. The effential oil of lemon or orange rind, produces repulfion, but weaker.

11. Convinced by the preceding experiments that every volatile fluid at the atmofpheric preffure, was capable of producing this repulfive movement, I employed liquid ammonia. I therefore wetted the plate in the ufual manner, and brought The affinity of ammonia for water prevented its repulfive action,

a drop

a drop of volatile alkali to the centre. What was my furprife to obferve, that the flighteft motion was not manifefted in the water; I however fufpected the caufe, and believed that this apparent anomaly was owing to the ammonia, which, having a very great affinity with water, combined with it at the inftant of their contact.

which was exercifed in olive oil.

12. I refolved, therefore, to fubftitute a fluid to the water, which had lefs affinity with ammonia, and I chofe olive-oil, which I had at hand. I again covered the bottom of the plate with a thin layer of this oil, and brought a drop of the ammonia to the centre: the oil was inftantaneoufly repelled, as it had been by the alcohol.

Ammonia does not repel alcohol:

13. If the plate be wetted with alcohol, and a drop of ammonia is put into the centre, the alcohol is not repelled, the drop of ammonia flattens, and the two liquors evaporate. I thought I perceived a flight tremulation on the edge of the drop of ammonia.

but alcohol repels ammonia.

14. If the plate be wetted with ammonia, and a drop of alcohol is brought to the centre, the ammonia is repulfed like pure water. It appears, therefore, from this experiment, and the preceding, that the horizontal or lateral force of expanfion of the alcohol is fuperior to that of the ammonia.

Olive-oil and water have no repulfive action.

15. A drop of olive oil put into a wetted plate, did not produce any motion in the water. It was the fame with water beat up with oil to the confiftence of an unguent. *M. Prevoft*, in his memoirs, feems to announce refults obtained with the fixed oils, which are contrary to my experiments, and particularly to thefe.

I fhall not enlarge farther on experiments which fome may, perhaps, think more curious than ufeful. But when the attractive powers of yellow amber, or of the loadftone were firft obferved, neither their importance, nor the aftonifhing difcoveries to which they have fince led, were fufpected.

XIV.

Letter from Mr. CUTHBERTSON *respecting his Galvanic and Electrical Experiments.*

To Mr. NICHOLSON.

DEAR SIR,

IN consequence of the note which you have been so obliging as to add to my letter, addressed to Dr. Pearson, inserted in your Journal for this month, I have to say, that *the troughs were used collaterally:* I shall now be happy to see your remarks. It appears, that I ought to have been more explicit, and therefore, I beg leave to offer the following additional observations *. Introduction.

The two last mentioned experiments in the letter alluded to, were compared with common electrical discharges, with a view to prove what quantity of coated glass would be requisite to ignite the same lengths of wire.

Two jars, each containing about 170 square inches of coated surface, were set to the conductor of a 24 inch single plate electrical machine, with my universal electrometer loaded with 31 grains, (see Quarto Journal, *Plate* XXII. Vol. II.) Experiment. Deflagration of wire by jars.

* I ought certainly to have mentioned the arrangement of the troughs, and likewise I ought not to have said so vaguely, that double quantities of galvanic fluid, only burn double lengths of wire, because I am strongly of opinion, that the reason why galvanic discharges from troughs do not act upon metals in the same ratio as common electric discharges do, proceeds from some defect in the arrangement, and also construction.

I find in my notes of improvements for the 6th of June, 1803, that I had made a pile of 16 pairs of plates of 10 inches diameter, and that eight of them laid upon each other in the usual manner, with cloths wetted with diluted muriatic acid, burned one inch of wire of 1-197th part of an inch in diameter, and that 16 pairs burned four inches of the same wire. This experiment was repeated on the 8th of June, with the same result, with respect to metals, but gave strong and loud sparks from metal to metal, sufficient to be heard at 200 yards distance, which result, I believe, has never been obtained from troughs, so as to be heard, indeed, at any distance. For the last experiment, the cloths were wetted in a strong solution of muriate of ammonia. Pile of large plates. Very loud galvanic sparks.

Eight

Eight inches of the fame fort of wire were laid in the circuit, 57 revolutions of the plate caufed the electrometer to difcharge the jars which ignited the wire perfectly, as in the ninth experiment. Then fix inches of the wire were laid in the circuit, and the above nnmber of revolutions caufed the difcharge, the wire being deflagerated and fufed into balls in the fame manner as in the eighth experiment.

Deduction as to the quantities of electricity in a jar and a pile.

Hence I conclude, that 340 fquare inches of coated glafs properly conftructed, will bear a charge equal to a galvanic battery of 1080 fquare inches of furface.

Probability that Mr. Wilkinfon's wire was thinner than ftated.

The refult of the above experiments gives me reafon to think, that there is a miftake refpecting the diameter of the wire ignited by Mr. Wilkinfon's batteries, as mentioned in your Journal, Vol. VII. p. 297, to which you refer, becaufe to ignite one half inch of fteel wire of *one feventieth of an inch in diameter*, will require a power fufficient to ignite 120 inches of wire $\frac{1}{150}$ part of an inch in diameter, by common electrical difcharges, which is a power equal to two of my common electrical batteries, (fee your Quarto Journal, Vol. II. p. 525.)

The greateft power of 60 pairs of 6 inches fquare plates that ever has been known, was that of igniting 16 inches of wire of $\frac{1}{150}$ part of an inch in diameter. Mr. Wilkinfon's trough of 100 pairs of plates of 4 inches fquare is of much lefs furface, and as he fays, it is a lefs favourable fize, from which, and from the above experiments, I conclude, that fuch a battery has not the power of igniting one half an inch of wire *of one feventieth of an inch in diameter*, unlefs galvanic difcharges act upon metals in fome manner different to common electrical difcharges, but with which I am unacquainted; perhaps Mr. Wilkinfon will be kind enough to clear up this remark.

I am, with due refpect,

Dear Sir, Your very humble Servant,

JOHN CUTHBERTSON.

Poland Street, Soho,
June 19, 1804.

XV.

Chemical Examination of the Ochroites, a Mineral not hitherto well known, containing a New Earth. By KLAPROTH *.

THE fossil which forms the subject of the present analysis, and to which I have given the name of ochroites, for reasons to be stated hereafter, is found in the mine of Basnætes, near Riddarhytta in Westmannland. History of the fossil.

The first account of this mineral we owe to Cronstedt, who furnished a description of it, together with that of another mineral, found at Bispberg in Delecarlien †. Scheele considered both as species of iron ores, and gives to them the name of lapis ponderosus, ponderous stone, (schwerstein) or tungsten, which he describes in his mineralogy as *terrum calciforme, terra quadem incognita intime mixtum* ‡. He likewise examined this tungsten, and made us acquainted with its true nature. The mineral which he examined, was, however, the pearl-coloured tungsten of Bispberg, and from this he concluded, that the examination of the tungsten of Riddarhytta was necessary, considering it a mineral of the same nature, he distinguished it by the name of *reddish tungsten.* First noticed by Cronstedt; and examined by Scheele

Soon afterwards D'Elhuyar analysed both minerals, he verified the analysis of the true tungsten, but proved that the conjecture of Scheele concerning the other mineral was founded in error; the results of his analysis showed that the fossil known by the name of reddish tungsten, was composed of 54 parts of lime, 24 of iron, and 22 siliceous earth. From what follows, it will, however, become obvious, that this mineral contains neither lime nor tungsten, but a new earth hitherto unknown. and D'Elhuyar.

External Characters of the Ochroites.

The colour of this mineral is between carmoisin red, clove-brown, and reddish-brown. It is compact, breaks, splinter- External characters of Ochroites.

* Gehlen's new Journal of Chemistry, Vol. II. part. iii. p. 303.

† Transactions of the Swedish Academy of Sciences, 1751. p. 235.

‡ And also in Cronstedt's Mineralogy by Magellen, Vol. I. p. 46.

ing

ing in irregular, not very ſharp or angular pieces. It is perfectly opaque. Its powder is reddiſh-gray. It is not very hard, but brittle, and very ponderous.

Its ſpecific gravity is 4,660. Cronſtedt ſtates it to be =4,938.

A.

Analyſis of the Ochroites.

a. A piece of the mineral after having been ignited to redneſs, loſt 2 per cent. Its reddiſh colour had been changed to brown. Its figure had ſuffered no alteration.

b. One hundred grains of the finely levigated mineral ignited for half an hour, loſt five grains. Its colour was changed to a dark brown.

B.

a. One hundred grains of ochroit, after being mixt with 200 grains of carbonate of potaſh, were ſtrongly ignited, the maſs which could not be rendered fluid, was reddiſh, grey and brittle. On being diffuſed through water, as uſual, the obtained ſolution was colourleſs. It remained perfectly tranſparent; a proof that it did not contain tungſten oxide; nitrate of ſilver, mercury, lead, barytes, &c. proved the abſence of acids.

b. The inſoluble reſidue of the laſt proceſs was boiled in nitro-muriatic acid, the ſiliceous earth being ſeparated, the ſolution was decompoſed by potaſh, and the whole boiled for ſome time. The alkaline fluid after being neutralized with muriatic acid, and then mingled with carbonate of potaſh, ſuffered no change.

C.

a. 200 grains of the finely pulverized mineral, were firſt boiled in two ounces of muriatic acid, to which half an ounce of nitric acid was gradually added, and the digeſtion continued for ſome time. The whole became thus diſſolved except the ſilex contained in the mineral. Its quantity amounted to 68 grains.

b. To the ſolution obtained in the laſt proceſs, carbonate of ammonia was added ſo long, till no permanent precipitate was produced. On letting fall into it ſuccinate of ammonia, a curdly precipitate fell, which vaniſhed again on agitation, leaving

leaving merely a pale red precipitate of fuccinate of iron. This being collected, wafhed, dried, and ftrongly ignited, yielded nine grains of oxide of iron.

c. The fluid thus freed from iron, and now colourlefs, was decompofed by carbonate of ammonia. The precipitate obtained was white, and weighed 168 grains, on being deprived of water and carbonic acid by heat, its white colour changed to cinnamon-brown. It weighed 109 grains.

d. All the water employed for wafhing the different precipitates were mingled, evaporated to drynefs, and the ammoniacal falt volatilized; a minute quantity of a muriate was obtained, the bafis of which could not be determined.

Peculiar earth. From what follows it will become evident, that the cinnamon-brown precipitate (c.) which forms the principal part of the foffil is a peculiar earth, diftinct from all the others hitherto known. The characteriftic property which it poffeffes of acquiring a light-brown colour after being heated, has induced me to call it ochroit earth*, which may alfo ferve for the mineral itfelf.

According to this analyfis, 100 parts of the ochroite of Ridderhytta contain,

Ochroit earth - - -	54,50
Silex - - - - -	34
Oxide of iron - - - -	4
Water, &c. (A. b.) - - -	5
Lofs - - - - - -	2
	100.

Characteriftic Properties of Ochroit Earth.

Ochroit earth, 1. combines with carbonic acid. 1. Ochroit earth is capable of combining with carbonic acid during its precipitation from acids by carbonated alcalies, and ftrongly confolidating a portion of water.

100 grains of the earth precipitated by carbonate of ammonia, and ftrongly dried, loft on being neutralized by nitric acid, 23 grains: 100 grains of the fame earth loft after being ftrongly ignited, 35 grains, 100 parts of carbonate ochroite therefore confifts of

Ochroit earth - - - -	65
Carbonic acid - - - -	23
Water - - - - - -	12
	100.

* From the Greek word ωχρος, (Havefcens,) brownifh yellow.

2. It is brown. 2. Ochroit earth, after being freed from carbonic acid and water, by heat, always appears in the form of a cinnamon-brown powder. The intenſity of the colour is in proportion to the heat applied. This colour is not owing to the preſence of iron, or mangaueſe, &c. but it is a characteriſtic property of the earth.

3. Not reducible. 3. Ochroit earth included in a charcoal crucible, and expoſed to the heat of the porcelain furnace, ſuffered no change whatever.

4. Not fuſible as the microcoſmic ſalt nor borax. 4. Urged by the blow-pipe, it becomes phoſphoreſcent; fuſed with phoſphate of ſoda and ammonia, it becomes tinged by it, without effecting a ſolution of the earth. The ſalt acquires merely a marbled lemon yellow colour. Borax has likewiſe no chemical effect upon it. This ſalt only effects a mechanical diviſion. The earth always appears diffuſed through the borax in minute *flocculi*.

5. Gives an uneven brown as a porcelain colour. 5. Ochroit earth mixed in different proportions with proper fluxes, and applied for painting of porcelain, proved unſucceſsful. The painted articles were light brown, but the colour was not uniform; a proof that no combination had been effected.

6. Difficultly ſoluble in acid if the earth be pure; but eaſily if carbonated. 6. Ochroit earth combined with carbonic acid is eaſily ſoluble with effervefcence in acids. The taſte of the ſolution is very rough and aſtringent. The concentrated ſolution is of an amethyſt red colour; diluted with water, it becomes colourleſs. Ignited ochroit earth, on the contrary, is difficultly ſoluble in acids in the cold; if nitric acid be employed, the ſolution is yellowiſh red.

Nitric acid.

7. Sulphate of ochroit is cryſtallizable, and pale amethyſt colour. 7. The combination of ochroit earth with ſulphuric acid, is cryſtallizable. The figure of the cryſtals formed in the maſs of the fluids is the octahedron. They are heavy, of a pale amethyſt colour, and difficultly ſoluble in water; but the ſulphate of ochroit with exceſs of acid, is more ſoluble; the figure of the cryſtals formed on the ſides of the veſſel, are needle-ſhaped, radiating from a centre. They are more ſoluble than the former.

Sulphate of ſoda decompoſes mur. or nitrate of ochroites. 8. If a ſolution of ſulphate of ſoda be mingled with a ſolution of muriate or nitrate of ochroit, a mutual decompoſition takes place. A white inſoluble precipitate is formed, conſiſting of ſulphuric acid united to the ochroit earth. This combination

The inſoluble ſulphate of ochroites.

bination may be decomposed by boiling it with double its weight of carbonate of soda. By this means ochroit earth may be obtained very pure.

may be decomposed by boiling with carb. of soda; and the earth obtained pure.

9. Ochroit earth is likewise soluble in sulphureous acid, the solution crystallizes in needles of a pale amethyst colour.

9. Sulphureous acid dissolves ochroit earth; crystallizable.

10. Muriatic acid dissolves ochroit earth, and yields crystals, the figure of which is the prism. It is soluble in alcohol without imparting to its flame any particular colour.

Muriatic acid also dissolves it, and crystallizes. The alcohol solution does not colour flame.

12. Acetite of ochroit could not be crystallized, but yielded an adhesive mass.

Acetite of ochroit not crystallizable.

13. Nitrate and muriate of ochroit is decomposable by carbonated earths and alcalies, the precipitate is milk-white. Alcalies and earths freed from carbonic acid, occasion a yellowish grey precipitate.

The nitrate and muriate precipitable by earths and alkalis,

14. Prussiate of potash precipitates ochroit from all its neutral solutions, milk white. The precipitate is soluble in muriatic and nitric acid.*

and by prussiate of potash; white.

15. Tincture of galls occasions no change in the solutions of this earth.

Not by galls,

16. Hidroguretted-hidrosulphuret of ammonia precipitates the solution of ochroit earth, yellowish white.

but by hidrog. hidro. sulph. of ammonia.

17. Water impregnated with sulphuretted hidrogen occasions no change in the solutions of ochroit earth.

Not by hidro. sulph. water,

18. Succinates precipitate ochroit earth white.

but by succinates.

19. Phosphate of soda, occasions in the solutions of this earth a white precipitate, which again vanishes by the addition of nitric or muriatic acid.

Phosphate of soda precipitates, &c.

20. Tartrites of potash also precipitate this earth white.

Tartrites precipitate it,

21. Oxalates effect a like decomposition, the oxalate of ochroit, however, is not soluble in nitric or muriatic acids.

and oxalates.

22. Alcalies and alcaline carbonates do not act on ochroit earth.

Alcalies do not act on the earth.

23. Ammonia feebly acts on it, under certain circumstances, as may be evinced from the following experiment:

Ammonia feebly.

A solution of nitrate of ochroit, prepared by dissolving 100 grains of carbonate of ochroit (not absolutely free from iron) in nitric acid, was decomposed by carbonate of ammonia, and digested in the fluid, containing a considerable quantity of carbonate of ammonia in excess, for some days. The fluid

* If the earth contained the muriates and quality of iron, it becomes by this means manifested.

which had acquired a yellow colour, was separated and neutralized by sulphuric acid, and then placed in a warm place. A grey precipitate was thus obtained, which on being dried, weighed $1\frac{1}{4}$ grains. This precipitate, after being dissolved in nitric acid, yielded a blue precipitate by prussiate of potash, this being separated, a white flocculent precipitate fell down by dropping into the remaining fluid carbonate of potash. This method is therefore applicable for separating a minute quantity of iron, that may be contained in the fluid.

General remarks and characters of the ochroit earth.

From what has been stated, it becomes obvious, that the ochroit earth bears the nearest relation to yttria, for like this it forms a connecting link between the earths and the metallic oxides. Like yttria it has the property of forming a reddish coloured salt with sulphuric acid, and is precipitable by prussiate of potash, but it differs from yttria, that it does not form sweet salts, that it is not (at least very sparingly) soluble in carbonate of ammonia, and that when ignited it acquired a cinnamon brown colour. It farther differs from yttria by not being soluble in borax or phosphate of soda when urged upon charcoal before the blow-pipe, which salts easily effect a solution of yttria, and melt with it also into a pellucid pearl.

XVI.

Letter from VAN MARUM *to J. C. Delamétherie, on Ritter's Galvanic Experiments.**

New experiment in proof of the identity of electricity and galvanism.

MR. ORSTED of Copenhagen, on his way through Harlem, having shewn me, by means of the apparatus in Teyler's Museum, some discoveries of his friend Ritter of Jena;† I made at the same time a new experiment with him, which affords a fresh proof of the identity of the fluid set in motion by Volta's pile and the common electrical machine, and an account of which I imagine will be acceptable for your Journal.

Two wires of platina, kept 5 minutes in the chain of communication between the two poles of the pile, convulsed the legs of a frog,

Mr. Orsted having shown me, that two wires of platina, after having been kept five minutes in the chain of communication between the two extremities of a galvanic pile, acquired

* *Journ. de Physique*, December, 1803, Vol. LVII. p. 471.

† See Ritter's experiments, as communicated by Dr. Orsted, in this present and some other numbers of the Journal.

thereby

thereby the faculty of throwing the legs and thighs of a frog into convulsive movements, according to the discovery of Mr. Ritter; I proposed to him, to pass the stream of fluid from a powerful electrical machine through these same wires of platina, in order to observe, whether it would have the same effect on them as the stream from the galvanic pile. We employed the plate machine of 35 inches diameter, and of the new construction, described in the *Journal de Physique* for June, 1791, vol. 38, (or Philosophical Journal, quarto series, No. 2.)* Having passed the stream from it through the wires of platina, held a quarter of an inch from the conductor, for the space of five minutes, we touched with them the crural nerves of a frog prepared in the usual manner, and observed immediately the same convulsive movements in its legs and thighs, though not so strong as in the preceding experiment. This less powerful effect of the stream from the machine perfectly answered my expectations, since my experiments in November, 1801, described in my letter to Mr. Volta, shewed me that the stream from the machine had not more than five sixths of the velocity or strength of the stream set in motion by a similar pile.

and exposed to the stream of a plate machine for a like time did the same, but more weakly.

We then repeated the experiment, keeping the wires of platina in contact with the conductor of the machine, while we passed the stream through them. Then holding the wires one in each hand, as in the preceding experiment, and in contact with the two crural nerves, but keeping their opposite extremities separate, their effect on the same frog, the sensibility of which was greatly weakened, was scarcely perceptible: but on bringing the upper extremities of the wires together, while the lower remained in contact with the crural nerves, we noticed very striking convulsions in the legs of the frog, every time the extremities were made to touch. A little sealing wax, which had been used to keep the wires of platina insulated when held to the conductor, and which still adhered to one of the upper ends, rendered the experiment still more evident; for when we brought these ends together in such a manner, that a little of the wax prevented them from being in perfect contact, the legs of the frogs exhibited none of the

The experiment was repeated, the wires having been kept in contact with the conductor; when the sensibility of the frog being much weakened, the effect was scarcely perceptible, unless the upper extremities of the wires were brought into contact.

When a little sealing wax prevented the absolute contact of the wires, no effect took place.

* It is a machine, that produces positive and negative electricity, like the pile of Volta. *Delamétherie.*

convulsive

convulsive movements, that took place, notwithstanding every time the wires were made to touch each other completely.

Thus we have a fresh proof of the identity of the fluid set in motion by the pile of Volta, and by a common electrical machine.

XVII.

Experiments on Light; by Mr. RITTER, *of Jena. Communicated by* Dr. ORSTED.*

Herschel's discovery of invisible solar rays has given rise to another.

THE important discovery of invisible solar rays, with which Herschel has enriched natural philosophy, has given occasion to another little known, even in the country where it has been made.

Our knowledge of light at a stand since Newton;

Our knowledge of light had made no perceptible advancement since Newton, when Herschel found, that all the phenomena occurring during the decomposition of light by means of the prism had not yet been noticed. Philosophers had contented themselves with perceiving different colours, without examining by other processes, whether phenomena imperceptible to the eyes did not take place. Herschel, by means of the thermometer, discovered invisible rays exterior to the solar spectrum, that possess the property of raising the mercury. Mr. Ritter repeated his experiments with success; but considering, that the different rays of light produce very different chemical changes in bodies susceptible of them, he conceived, that light likewise contained invisible rays, which acted chemically. He exposed muriate of silver to the action of the solar spectrum, and found his conjecture substantiated: the muriate of silver very soon became black beyond the violet edge of the spectrum; blackened a little less in the violet itself; and this action was still less in the blue, diminishing thus more and more in proportion to the distance from the violet, till it became null. On exposing muriate of silver a little blackened, that is to say, a little disoxigenated, to the same action of light, its white colour was partly restored by the red ray, and still more by the invisible ray beyond it.

till Herschel found, that invisible rays, beyond the spectrum, raised the thermometer. Ritter, repeating the experiment, conjectured, that light contained invisible rays acting chemically likewise; and found, that muriate of silver, exposed to the spectrum, grew blackest beyond its violet edge, and the effect diminished in proportion to the distance from this, and the muriate a little disoxigenated had its colour partly restored by the in-

* *Journal de Physique*, December, 1803, Vol. LVII. p. 409.

The

The solar spectrum therefore is accompanied by two invisible rays, one on the red side, which favours oxigenation, and the other on the violet side, which favours disoxigenation. The visible and coloured rays partake more or less of the properties of these invisible principles, whence we ought to infer, that all the coloured rays contain more or less of these principles.

Visible ray beyond the red. Thus there is an invisible ray promoting oxigenation, and another the contrary, and the visible and coloured rays partake more or less of the properties of both.

These experiments succeed very well with phosphorus. On letting the invisible ray adjoining the red fall on it, it immediately emits white fumes: but if the invisible ray adjoining the violet be thrown on the phosphorus in a state of oxigenation, it is instantly extinguished, with the same rapidity as a frog is convulsed in galvanic experiments.

The invisible ray on the red side likewise kindles phosphorus; while that on the violet side extinguishes it instantly.

These experiments readily accord with some others made by the same gentleman. He kept one of his eyes for some minutes in contact with the negative conductor of Volta's pile, and after this operation all objects appeared red to him: but after having kept it in contact with the positive conductor, he saw every thing blue. It is to be observed here, that the retina and optic nerve, when the external part of the eye is brought into the negative state, become positive, and *vice versa*; because the eye is filled with a fluid, in which the same distribution of electricity must take place, as in water and other fluids. It is in the positive state therefore, that the optic nerve perceives all objects of a red colour; and in the negative state they appear violet. The chemical action of positive electricity likewise is the same as that of red light; that is, they both favour oxigenation. Negative electricity and the violet ray possess the same analogy, both promoting disoxigenation: as the experiments with the galvanic pile have sufficiently shown.

One of the eyes being kept some minutes in contact with the negative conductor of Volta's pile, all objects appeared red to it; in contact with the positive blue.

In the former case the optic nerve is in the positive state; in the latter, negative.

The chemical action of positive electricity likewise favours oxigenation, like red light; and negative, the contrary.

If I might be allowed to add any observation to these important discoveries, I would mention one of the most commonly known facts; that the opposite electricities, when united, produce light; which seems to demonstrate synthetically, what the preceding experiments have shewn by analysis.

The two electricities too produce light by their union; which seems to prove synthetically their identity with the chemical rays.

This account was read to the philomathic society several months ago, since which time Mr. Ritter has published some new observations that deserve notice. He has found, with all the prisms he used, that the solar rays give two coloured spectrums, which spread in proportion to their distance from the prism, so that at a certain distance one nearly covers the other. The

Mr. R. has since found, that the prism affords two spectrums,

distinct at a little distance, as 4 inches, but confounded together when farther off.

The experiment must be made at a very small distance from the prism, that of four inches for example, to distinguish the two spectrums, which become confounded together in proportion to their distance from the prism. No doubt this is the reason why the phenomenon has so long escaped the notice of philosophers. This observation was accompanied by another, still more important; namely, that the chemical rays may be completely separated from the coloured rays. If the invisible rays of the violet side be made to fall on the red part of the solar spectrum, the process of oxidation may be completely suspended there, and even disoxidation produced, without destroying the red colour; and by means of several prisms we may even separate all the coloured rays from the chemical rays. We may thus produce a coloured spectrum devoid of chemical action; and a series of chemical rays, analogous to the spectrum, without any mixture of coloured rays.

And the chemical rays may be completely separated from the coloured, without destroying them.

So that we may produce a coloured spectrum devoid of chemical action.

We have not yet any accurate experiments, to determine whether the calorific rays be likewise separable from the others, particularly from the chemical rays: but the comparison of different experiments made in winter and summer, when the degrees of heat are different, though no difference in the force of the chemical rays at different seasons has been observed, lead to a belief that the calorific rays are separable from the chemical.

The calorific rays have not been proved to be separable from the chemical; but it may be presumed they are, since the former differ in degree in winter and summer, the latter do not.

It may be asked, why the different rays found in light frequently accompany each other, though they can subsist separately: and this question no doubt may be answered, when we are able to say, why the different functions of electricity accompany each other, though they likewise are separable.

That they accompany each other, though separable, no greater difficulty than attends electricity.

XVIII.

On Spontaneous Inflammations. By G. C. BARTHOLDI, *Professor of Physic and Chemistry.**

Definition of spontaneous inflammation.

SPONTANEOUS inflammation is that which is manifested in a combustible body, without the immediate contact of any ignited matter.

* From the Annales de Chimie, Vol. XLVIII. p. 249, or No. 144.

These

These kinds of combustion may be occasioned by different causes, the principal of which are: Causes.

1. Considerable friction.

2. The action of the sun.

3. The disengagement of caloric, produced in bodies, which, though not combustible, by being brought near to other combustible bodies, may communicate such a degree of heat to them, that they inflame by the contact of the air.

4. The fermentation of animal and vegetable substances heaped up in a large mass, which are neither entirely dry, nor too wet, such as hay, dung, &c.

5. The accumulation of wool, cotton, and other animal and vegetable substances, covered with oily matter, particularly drying oil.

6. The preparation of linseed oil for printers' ink, of varnishes, and in general of all fat.

7. The torrefaction of different vegetable substances.

8. The sulphurated and phosphorated hidrogen gases which are disengaged in many of the operations of nature, and of which, the latter generally inflames by the sole contact of atmospheric air, even at a low temperature, and which is often seen at the surface of the earth like a small flame, known by the name of Jack o'Lantern, in places in which there are animal substances in a state of putrefaction: if other combustibles are met with where the disengagement takes place, they readily catch fire.

9. The phosphuret of lime and potash, which may be formed in the preparation of charcoal, particularly in that from turf, and from some sorts of wood which grow in marshy situations. This charcoal by being wetted, or by simply attracting the humidity of the air, forms phosphureted hidrogen gas, which, by the contact of the atmospheric air inflames, and may set fire to the whole mass of charcoal.

10. The phosphorus which is sometimes, though rarely, formed in the carbonisation of different sorts of wood, without combining either with lime or with potash in the state of phosphuret. These charcoals do not inflame spontaneously at the common temperature of the atmosphere, but they produce a detonation by percussion with nitrate of potash, or with some other nitrates and metallic oxides to which the oxigen adheres but feebly, and which, being in the state of thermoxide, retain much latent caloric.

1. *Friction.*

Friction.

It is generally known that by rubbing two bodies against each other, they are heated; the intenfity of the produced heat depends on feveral circumftances, and chiefly varies in proportion to th. hardnefs of the friction, to the nature, and to the furface of the rubbing bodies: if the friction takes place between combuftible bodies, fuch as the woods, the heat which it excites is frequently fufficient to inflame them; if the bodies are not combuftible, fuch as ftones, or but little combuftible, like the metals, they do not themfelves inflame, but they can communicate fuch a degree of heat to other combuftibles which furround them, that thefe can inflame by the contact of atmofpheric air.

Dr. Palcani having repeated the experiments which have been long known for obtaining fire by the friction of two pieces of wood, to one of which he gave the form of a tablet, and to the other that of a fpindle or a cylinder, he has allowed me to give the refults of fome of his experiments here, to fhew that more attention fhould be paid to the choice of the woods which are deftined to rub againft each other, in the conftruction of machines and inftruments.

Experiments with a tablet between two cylinders,

Two cylinders of		Tablet.		Duration.	Effect.
Box-wood	rubbed againft	Box-wood	during	5 Minutes	Senfible heat
Idem		Poplar		*Idem*	*Idem*
Idem		Oak		*Idem*	*Idem*
Idem		Mulberry		3 Minutes	Cons. heat and fmoke
Idem		Laurel		*Idem*	*Idem*
Laurel		Poplar		2 Minutes	*Idem*
Idem		Ivy		*Idem*	*Idem*
Ivy		Box-wood		3 Minutes	*Idem*
Idem		Hazle		*Idem*	*Idem*
Olive		Olive		*Idem*	*Idem*
Mulberry		Laurel		2 Minutes	Con. he. fm. & black
Afh		Oak		5 Minutes	Senfible heat
Idem		Fir		*Idem*	*Idem*
Pear-tree		Oak		*Idem*	*Idem*
Cherry-tree		Elm		*Idem*	*Idem*
Plum-tree		Apple-tree		*Idem*	*Idem*
Oak		Fir		*Idem*	*Idem*

In

On changing the experiment, and rubbing a cylinder of one of these woods between two tablets of the other, for example, a cylinder of poplar between two tablets of mulberry, the augmentation of the rubbed surfaces which are in contact with the air, produced a much more considerable heat, and nearly all the woods mentioned above took fire. and with a cylinder between two tablets.

The effect of the friction also varies according as the woods employed, particularly if they are of the same species, are rubbed in the direction of the grain of the wood, or when the grains of the woods cross each other. In the first case the friction and the heat are much more considerable than in the second. The direction of the grain of the wood affects the friction.

In large machines, in which there is a great deal of friction, the heating is prevented by directing a continual current of cold water on the rubbing surfaces: in common machines, and in coaches, waggons, &c. it is diminished by covering the rubbing surfaces with some oily matter. There have been many examples, during the great heat of summer, of coaches, and other machines subjected to a rapid motion, having taken fire, because the greasing them had been neglected. The grease, by hardening on the rubbing surfaces, instead of diminishing the friction, increases it; and as this covering is very combustible, it renders spontaneous inflammation still more easy. It is also preferable, in many circumstances, to rub machines with soap, talc, plumbago, or other substances, which, without being oily, are very unctuous to the touch. Preventives against spontaneous inflammation by friction.

2. *Action of the Sun.*

By concentrating the solar rays with convex glasses or concave mirrors, the strongest heat is produced, all sorts of combustibles are set on fire, and the most refractory substances are melted: it may happen that other bodies may be found in circumstances, in which, without our concurrence, they produce the effects of glasses and burning mirrors: although these effects are rather physical than chemical, it is nevertheless essential to make them known, to guard against their danger. There are examples of fires produced by large glass bottles, filled with water and exposed to the sun, in an apartment. Whenever the form of the vessel is nearly similar to that of a Action of the sun.

lenticular

lenticular or spherical glass, the rays are refracted, and by uniting in a focus, produce a heat capable of setting fire to the combustible bodies within it.

3. *The Heat excited in Bodies not combustible.*

Heat from incombustible bodies.

It is known that quick-lime plunged into water, or simply moistened, produces a considerable heat. This method has even been employed with success for heating apartments, green-houses, hot-beds, *&c.* at little expence. This property of quick-lime of disengaging much heat by the contact of water, and that, not less dangerous, of dissolving or corroding animal substances immersed in it, require the greatest precautions when a considerable quantity of quick-lime is kept together. To preserve it, it must be protected from the contact of the air and of every species of humidity, and it must be carefully kept at a distance from all combustible bodies, such as wood, hay, straw, *&c.* which might inflame spontaneously, if the lime contracted the least humidity. The *Journal de la Haute-Saône* gave an account, last year, of the destruction of a barn, one of the wooden partitions of which took fire from a heap of quick-lime, intended for the repairs of the farm, having been carelessly laid against it.

In nature a great number of analogous phenomena occur, in which bodies, by changing their composition, or by contracting new combinations, heat so much, or disengage such a quantity of caloric, that other combustibles which are near them may take fire.

(To be continued.)

XIX.

Discovery of two new Metals in crude Platina. By SMITHSON TENNANT, *Esq. F. R. S.*

New metal *Iridium.*

AT the last meeting of the Royal Society a paper of Mr. Tennant was read, on the analysis of the black powder which remains after dissolving platina, shewing that it contains two new metals. Mr. Tennant's first experiments were made last summer,

summer, and had been communicated to Sir Joseph Banks, after which an account of one of these metals appeared in France, by M. Descotil *, and also by M. Vauquelin. The properties ascribed to it by the French chemists are, 1. That it reddens the precipitates of platina made by sal ammoniac ; 2. That it dissolves in marine acid ; 3. That it is precipitated by galls and prussiate of potash. The properties mentioned by Mr. Tennant are, that it dissolves in all the acids, but least in marine acid, with which it forms octahedral crystals. The solution with much oxigen is deep red, with a smaller proportion green or deep blue. It is partially precipitated by the three alkalies when pure. All the metals, excepting gold and platina, precipitate it. Galls and precipitate of potash take away the colour of this solution, but without any precipitate, and afford an easy test of its presence. The oxide therefore loses its oxigen, by water alone. When combined with *gold or silver, it cannot be separated by the usual process of refining these metals.* As the French chemists have not given a name to the metal, Mr. Tennant inclines to call it *Iridium*, from the various colours of it in solution.

New metal *Osmium.*

The second new metal is obtained by heating the black powder with pure alkali in a silver crucible. The oxide of this metal unites with the alkali, and may be expelled by an acid and obtained by distillation, being very volatile. The oxide has a very strong smell, from which Mr. Tennant has called it *Osmium.* It does not redden vegetable blues, but stains the skin of a deep red or black. The oxide in solution with water has no colour, but by combining with akalli or lime becomes yellow. With galls it gives a very vivid blue colour. All the metals, excepting gold and platina, precipitate this metal. If mercury is agitated with the aqueous solution of the oxide, an amalgum is formed, which, by heat, loses the mercury, and leaves the osmium pure as a black powder.

* See our Journal, Vol. VIII. p. 118.

SCIENTIFIC NEWS, AND ACCOUNT OF BOOKS.

Figure of the Orbits of the new Planets. By JEROME DE LALANDE.

Figure of the orbits of the new planets.

THE mean distance of each of these planets from the sun is 2,77, which answers to 227 million of geographical miles nearly.

Piazzi or Ceres, discovered Jan. 1, 1801.

Revolution 4 years, 7 months, 10 days.

Mean longitude, Jan. 1, 1804 -	10s.	11°	59′
Annual motion - -	2	18	14
Aphelion - - -	10	26	44
Node - - -	2	21	6
Equation of the orbit - -		9	3
Eccentricity - - -		0,	079
Inclination - - -		10	37

Albers or Pallas, discovered March 28, 1802.

Revolution 4 years, 7 months, 11 days.

Mean Longitude, Jan. 1, 1804 -	9s.	29°	53′
Annual motion - -	2	16	11
Aphelion - - -	10	1	7
Node - - -	5	22	28
Equation of the orbit - -		28	25
Eccentricity - - -		0,2463	
Inclination - - -		34	39

Philosophical Transactions of the Royal Society of London, for the Year 1804. *Part I.* 4to, 182 *Pages, with five Plates and* 26 *Pages of Metereological Journal.*

Philosophical Transactions of the Royal Society.

THIS Part contains—1. The Bakerian Lecture, Experiments and Calculations relative to Physical Optics; by Thomas Young, M. D. F. R. S. 2. Continuation of an Account of a peculiar Arrangement in the Arteries distributed on the Muscles of slow moving Animals, &c.; in a Letter from Mr. Anthony Carlisle to John Symmons, Esq. F. R. S. 3. An

3. An Account of a curious Phenomenon obferved on the Glaciers of Chamouny; together with fome occafional Obfervations concerning the Propagation of Heat in Fluids; by Benjamin Count Rumford, V. P. R. S. Foreign Affociate of the National Inftitute of France, &c. &c. 4. Defcription of a triple Sulphuret of Lead, Antimony, and Copper, from Cornwall; with fome Obfervations upon the various Modes of Attraction which influence the Formation of Mineral Subftances, and upon the different Kinds of Sulphuret of Copper; by the Count de Bournon, F. R. S. and L. S. 5. Analyfis of a triple Sulphuret of Lead, Antimony, and Copper, from Cornwall, by Charles Hatchett, Efq. F. R. S. 6. Obfervations on the Orifices found in certain poifonous Snakes, fituated between the Noftril and the Eye; by Patrick Ruffell, M. D. F. R. S.: with fome Remarks on the Structure of thofe Orifices, and the Defcription of a Bag connected with the Eye met with in the fame Snakes; by Everard Home, Efq. F. R. S. 7. An Enquiry concerning the Nature of Heat, and the Mode of its Communication; by Benjamin Count Rumford, V. P. F. R. S. Foreign Affociate of the National Inftitute of France, &c. 8. Experiments and Obfervations on the Motion of the Sap in Trees; in a Letter from Thomas Andrew Knight, Efq. to the Right Hon. Sir Jofeph Banks, Bart. K. B. P. R. S. Appendix, Metereological Journal kept at the Apartments of the Royal Society, by Order of the Prefident and Council. Philofophical Tranfactions of the Royal Society.

Analytical Effays towards promoting the Chemical Knowledge of Mineral Subftances. By MARTIN HENRY KLAPROTH, *Profeffor of Chemiftry, Affeffor to the Royal College of Phyficians, Member of the Royal Academy of Sciences at Berlin, and various other learned Societies. Vol. II. 8vo, 267 Pages. Tranflated from the German.* Cadell and Davies.

THIS work is tranflated by the fame learned chemift to whom the fcientific world is obliged for the former volume. Analytical Effays. Its valuable contents are as follow. 73. Examination of the Auriferous Ores from Tranfylvania. 74. Analyfis of the fulphated Oxyd of Manganefe from Tranfylvania. 75. Examination of Tungftate of Lime (Scheelaim). 76. Gadolinite. 77. Examination of the Egyptian Natrum (Soda). 78. Striated

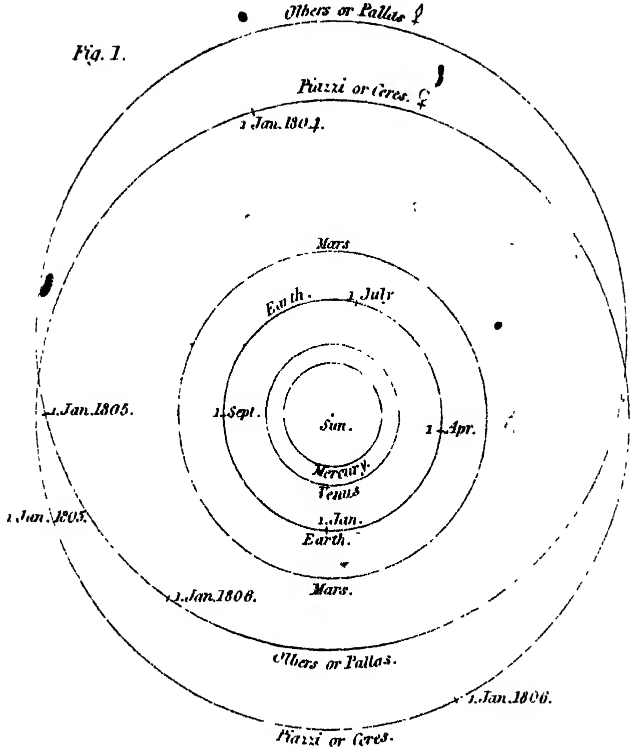
Analytical Essays.

ated Soda. 79. Analysis of the native Muriate of Ammoniac. 80. Examination of Saffolin. 81. Examination of the Plumose Alum from Freyenwalde. 82. Capillary Salt (Halotrichium) from Idria. 83. Elastic Bitumen, from Derbyshire. 84. Examination of Mellilito. 85. Umbra (Umber). 86. Examination of the muriated Lead Ore. 87. Phosphated Lead Ores. 88. Sulphated Lead Ores. 89. Tabular, White Lead Ore, from Leadhills. 90. Examination of the native Reguline Antimony, from Andreasberg. 91. Antimoniated Silver, from Andreasberg. 92. Fibrous *red* Antimonial Ore. 93. *White* Ore of Antimony. 94. *Arseniated* Olive Copper Ore. 95. *Muriated* Copper Ore. 96. *Phosphated* Copper Ore. 97. *Kryolite*. 98. Beryl. 99. Emerald. 100. Examination of Klingstone (Echodolite). 101. Basalt (Figurate Trapp). 102. Pitch Stone. 103. Addition to the Analysis of Pumice Stone (Essay 33). 104. Examination of the Jargon (Zircon) from Norway. 105. Examination of Madreporite. 106. Pharmacolite. 107. Scorza. 108. Examination of the Fibruous Sulphate of Barytes. 109. Tabular Spar (Safel-spath). 110. Examination of Miemite. 111. Examination of the *prismatic, Magnesian Spar*, from the Territory of *Gotha*. 112. Examination of the striated grey Ore of Manganese. 113. Earthy, black Oxyd of Manganese. 114. Examination of the Asphaltum from Albania. 115. Earthy brown Coal. 116. The Hungarian Pearl Stone.

*** W. N. acquaints G. S. that the supposition of Du Hamel, that what was called mild volatile alkali contains chalk, arose from a want of knowledge of the carbonic acid at the early period alluded to in his letter. The additional weight in the carbonate of ammonia arises from carbonic acid, and there is no reason to suspect any volatilization of the earth during its preparation. If any were present, it might be detected by sulphuric acid, or, in preference, by oxalic acid, either of which would carry down the lime.

Sketch of the Orbits of the new Planets, by Jerome de la Lande.

Fig. 1.



Arseniated Copper

Fig. 3.

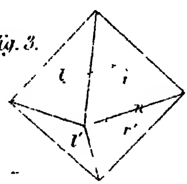


Fig. 2.

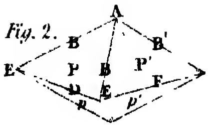


Fig. 4

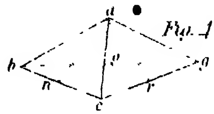
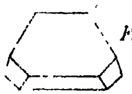
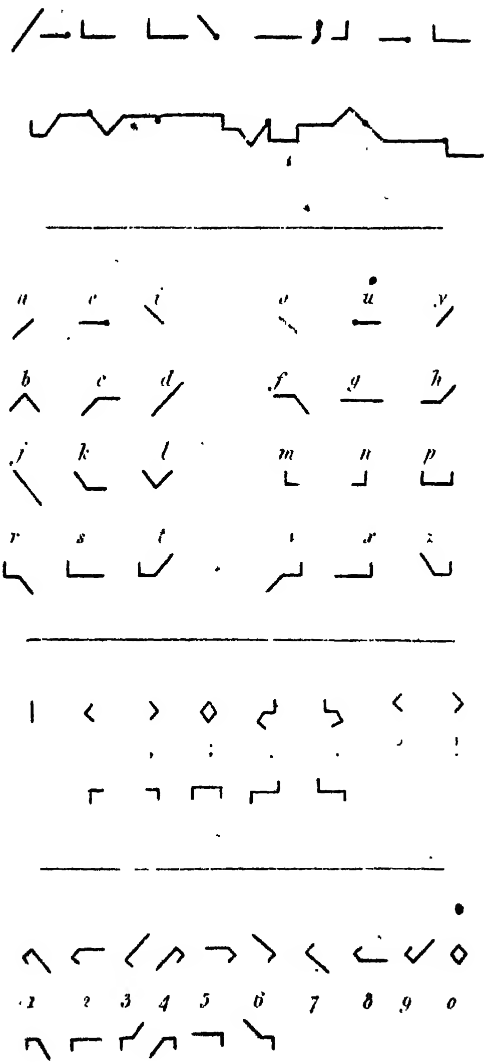


Fig. 5.



Telegraph by the human figure.



Mutlow sc. Russell Ct.

Machine for laying Land level, by Mr David Charles.

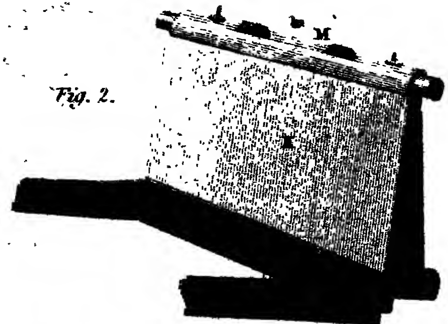


Fig. 2.

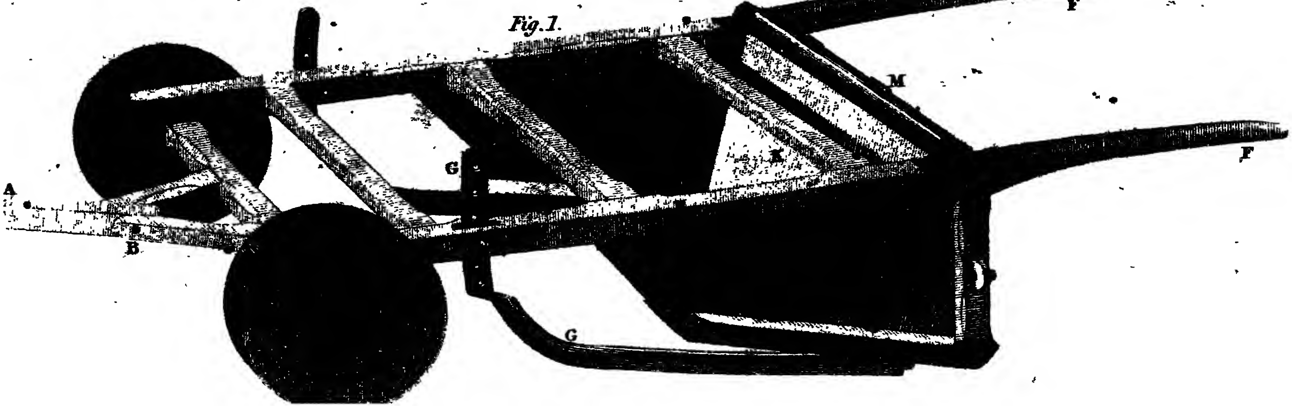
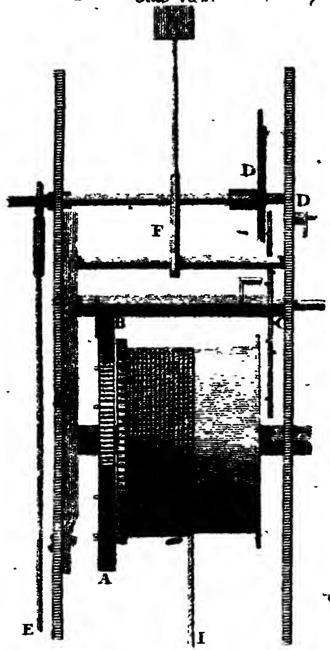


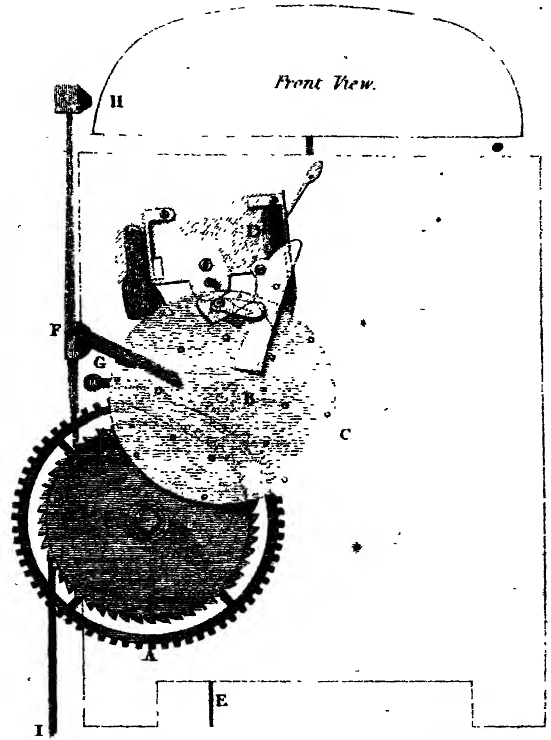
Fig. 1.

Clock to strike without a Fly,
by Mr. E. Massey.

Side View.



Front View.



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

AUGUST, 1804.

ARTICLE I.

On the Pastes, coloured Glasses, or Enamels, of the Ancients. *By M.* KLAPROTH.*

SECTION FIRST.

THE invention of glass, which, in various respects is so highly valuable a production of art, is among those ancient discoveries of which history has transmitted us some account. Pliny relates it as follows: † "*Fama est, adpulsa nave mercatorum nitri, cum sparsi per littus epulas pararent, nec esset cortinis attollendis lapidum occasio, glebas nitri e nave subdidisse. Quibus accensis, permixta arena litoris, translucentes novi liquoris fluxisse rivos; et hanc fuisse originem vitri.*" If we suppose this account to be merely an unsupported tradition, still it contains in itself no circumstance that might render it questionable. It deserves rather the more credit, as it is hardly possible to imagine this invention could have had any other origin than that of accident.

History of the invention of glass.

Pliny's Account.

* Read in the Royal Academy of Sciences at Berlin, October 4, 1798.—Translated by N. N. who received a copy from the author.

† Lib. XXXVI. Cap. 65.

Though Pauw and ſome other antiquaries are more inclined to aſcribe this diſcovery to the Egyptians, who are ſaid to have conſtructed the firſt glaſs-houſe, in the remoteſt ages, at Dioſpolis, the ancient capital of Thebaïs; yet it likewiſe appears, from the writings of the ancients, that this art muſt have arrived at a conſiderable degree of perfection, chiefly among the Phenicians; as alſo, in general, this nation ſeems, in her flouriſhing age, to have been almoſt in the excluſive poſſeſſion of manufactures. Sidon, that colony of theirs ſo flouriſhing by commerce, arts, and manufactures, was not leſs famous on account of her glaſs-houſes. Theſe, according to the teſtimony of Pliny, obtained for ſome hundred years the chief ingredients of their glaſs from the ſea-ſhore near the Phenician town Acco, afterwards called Ptolemais, and now St. John d'Acre, in the vicinity of the ſmall river Belus, which there empties itſelf into the Mediterranean.

Glaſs-houſes at Dioſpolis in the moſt remote ages,

and among the Phenicians.

The ſubſtance which the ancients employed for the purpoſe of vitrifying the ſand, is comprehended by the early authors under the name of *nitrum*; but it has long been generally agreed, that they did not mean by it our nitrate of potaſh, but the mineral alkali or ſoda; conſequently their *nitraria* were not nitre-works, but, ſtrictly ſpeaking, refineries of ſoda. And from the deſcriptions which Pliny and others have given of their *natrum* and its properties, it is rendered probable, that in thoſe times all ſaline ſubſtances, whether efflorеſcing upon the ſoil or left by dried lakes, if not belonging to the muriatic genus, were conſidered as natrum. Hence undoubtedly, among thoſe ſalts often occurred real nitre as well as native ſulphate of ſoda. However, ſuch confuſion in their uſe in manufacturing glaſs has not produced any real detriment; ſince the longer time during which the ancients expoſed their materials for glaſs to the action of the fire, has been more than ſufficient to decompoſe thoſe neutral ſalts, and to expel from them their acid conſtituent parts.

The nitrum or natrum of the ancients was ſoda;

or rather it conſiſted of any ſalt left upon the ground (not muriatic);

This impure ſalt was not detrimental to the glaſs.

The art of colouring glaſs ſeems to be of nearly the ſame antiquity as the invention of making it; as is evident not only from ſeveral paſſages in the ancient writers, but may alſo be proved by actual documents, and, among others, by the variouſly coloured glaſs-corals, with which ſeveral of the preſerved Egyptian mummies are decorated. This art ſuppoſes the poſſeſſion of ſome chemical knowledge of the metallic oxides, becauſe theſe are the only

The art of colouring glaſs is of nearly the ſame antiquity as glaſs itſelf.

only fubftances capable of producing fuch an effect. But it would be a problem of difficult folution to determine, what were the means and proceffes employed by the ancients for this purpofe, as they had no acquaintance with the mineral acids, which, at prefent, are ufually employed in the preparation of metallic oxides. It is neverthelefs certain, that the art of giving many various colours to glafs muft, at leaft in later times among the Greeks and Romans, have reached an eminent degree of perfection; for they knew how to imitate, by their paftes of glafs, even thofe gems which have a deep colour, fo as to deceive the eye very confiderably. A proof of this, among others, is afforded by the following words of Pliny *, relating to the artificial imitation of the *carbuncle*, a gem then in the higheft eftimation: "*Adulterantur vitro fimillime: fed cote deprehenduntur, ficut aliæ gemmæ factitiæ.*"

Though the ancients were ignorant of our mineral acids, they made coloured paftes in high perfection.

It was in the time of Auguftus that the Roman architects began to make ufe of coloured glafs in their mofaic decorations, befides the feveral fpecies of marble and other coloured ftones, which, before, were ufually employed with that defign. Such an application of the glafs-paftes was reforted to in a villa built by the emperor Tiberius on the ifland of Capri, as is fhewn by fpecimens lately found among its ruins. I fubjected fome of thefe in my poffeffion to chemical analyfis, chiefly for the purpofe of difcovering what metallic fubftances the ancients employed to tinge thofe varioufly coloured maffes of glafs.

Roman mofaic work of glafs.

I. *Antique Red Glafs.*

The colour of this glafs-pafte is a lively copper-red. The mafs is perfectly opake, and very bright at the place of recent fracture. This is probably the very fame glafs, of which Pliny fays: * "*Fit et totum rubens vitrum, atque non tranflucens, Hæmatinon adpellatum.*"

Antique red glafs; opake.

(*a*) *Two hundred* grains of this red glafs were finely triturated, and, together with 400 grains of *cauftic potafh*, ignited for half an hour; by which management the mixture foon entered into a thin fufion. After cooling, the whole mafs was foftened with water, then fuperfaturated with *muriatic* acid; and, after this mixture had been again infpiffated to a faline mafs,

Analyfis, 200 grains, triturated and fufed with 400 gr. potafh; foftened with water; fuperfaturated with muriatic acid; infpiffated; diffufed in much hot water.

* Lib. XXXVII. Cap. 26.

mafs, it was again diffufed in a large quantity of boiling water, to which a flight portion of muriatic acid had previoufly been added. *Siliceous earth* feparated; which, collected, edulcorated and ignited, weighed 142 grains.

Silex fell down.

The remaining folution depofited cryftals of muriate of *lead* by evaporation.

(*b*) The filtered folution poffeffed a green colour; and when concentrated by evaporation, it depofited needle-fhaped cryftals. When on continuing the evaporation no more fuch cryftals would appear, the remaining fluid was diluted with fpirit of wine, and thrown upon the filter. The collected cryftals were wafhed with fpirit of wine and dried in a warm temperature, upon which they weighed $32\frac{1}{2}$ grains. They confifted of muriated lead, equivalent to 28 grains of gently ignited *oxide of lead.*

The liquid was then fuperfaturated with ammonia, and *alumine* was feparated;
The rem. fluid being fat. with mur. acid, then evap. and precip. by oxalate of potafh, gave ox. of *lime.*

(*c*) I then fuperfaturated with *cauftic ammonia* the folution thus freed from its contents of lead. It was now of a dark-blue colour, and let fall a grey precipitate; which being feparated, the folution was again neutralized with muriatic acid, reduced by evaporation, and upon this combined with *oxalate of potafh* as long as any turbidnefs enfued. The precipitate thence formed was oxalate of lime, which after ftrong ignition yielded three grains of *pure calcareous earth.*

Copper was then precip. by *iron.*

(*d*) The ingredient copper was now precipitated from the folution, by immerfing into it a polifhed piece of iron. The reguline copper obtained by this procefs amounted to twelve grains, for which fifteen grains of oxided copper muft be put in the account.

Purification of the precipitated alumine.

(*e*) The above grey precipitate (*c*) thrown down by the cauftic ammoniac, was mixed and digefted with liquid *cauftic foda.* When to the filtered folution, again fuperfaturated with muriatic acid, *carbonate of foda* was added, *aluminous earth* fell down, which after wafhing and ignition amounted to five grains.

Infoluble part was *iron.*

(*f*) The remaining part that was left undiffolved by the cauftic lye, appeared of a black-brown tinge. This, after wafhing and expofure to red heat, weighed two grains, and was *oxided iron.*

Hence, according to this analyfis, the fum of the conftituent parts of the *two hundred* grains of the *red antique glafs-pafte* decompofed, confifts of,

Silex

Silex	(a)	142 grains.	Composition of the red glass.
Oxide of Lead	(b)	28 ——	
—— *of Copper*	(d)	15 ——	
—— *of Iron*	(f)	2 ——	
Alumine	(e)	5 ——	
Lime	(c)	3 ——	
		195	

On comparing the external characters of this red glass-paste with the cupreous scoriæ of a lively brown-red, such as is sometimes obtained on melting copper ores, it is rendered highly probable, that the ancients did not compound the above paste directly from its simple constituent parts, but instead of them have perhaps employed copper scoriæ. On that supposition they had nothing more to do, than to select the best coloured pieces to fuse and cast them into plates. It was probably a scoria from copper works.

II. *Antique Green Glass-Paste.*

The colour of this green paste is a light verdigris. Its mass, like that of the preceding, is opake, and of a scoriaceous splendent fracture. Antique green glass; opake. Analysis as in §. 1.

For its chemical analysis I employed *two hundred* grains, which, having been treated in exactly the same manner as the foregoing, I found to consist of the following ingredients:

Silex	130 grains.	Component parts of the green enamel:
Oxide of Copper	20 ——	
—— *Lead*	15 ——	
—— *Iron*	7 ——	
Lime	13 ——	
Alumine	11 ——	
	196 grains.	

This green glass-paste, then, contains the same constituent parts as the red, only in different proportions. Both receive their colour from copper: But the reason why this metal produces in the one a red, and in the other a green colour, depends on the different degrees of its oxidation or saturation with oxigen. Differ only in proportion of parts from the green.

It is one of the chemical properties of copper, that in the state of a sub-oxide, that is only half saturated with oxigen,

 it

it produces a copper-red enamel; while, on the contrary, when perfectly oxided or fully saturated with that acidifying principle, the enamel which it yields is green. Pliny mentions several preparations of copper that were in use in his time; he only dwells too long on enumerating their pretended medicinal virtues. Of such artificial preparations of copper some might have been serviceable in making green glass-pastes, in the case that, perhaps, the native oxides of copper, of which in particular the copper-mines on the island of Cyprus could afford copious quantities, were not then employed for this purpose.

III. *Antique Blue Glass.*

Whether the ancients coloured glass by cobalt.

My leading object in chemically decomposing this glass, was the solution of the question: What was the colouring matter which the ancients employed in order to tinge their glass blue? The striking similarity of the colour of the blue antique glass to that of our modern, which, as is well known, is tinged by means of cobalt, has induced several learned men to conjecture, that even the ancients must have been acquainted with this fossil, as well as with its properties of colouring glass blue. This was, likewise, the opinion of Ferber, when in his *Letters from Italy*, page 114, he says: "*In the villa Adriani near Tivoli, near Frescati, and in several places, antique musaic works have been found which exhibited some cubes of a blue vitreous composition, and serve as a proof that the ancients must have known the use of cobalt and the preparation of smalt.*" This opinion he repeats in various places.

The affirmative generally,

but erroneously supposed.

This opinion being supported by no chemical proof, rests solely on the supposition, that cobalt is the only substance which is capable of affording a blue enamel. However, it is certain the ancients knew the art of giving, by means of iron, a blue colour to glass resembling that which we produce by cobalt.

The contrary shewn by Gmelin,

A chemical demonstration of this fact has been given by Gmelin of Göttingen, in his *Chemical Examination of a Blue Glass from an antique Mosaic Fragment* *, which was found in digging a garden at Mümpelgard, and is probably of Roman

* Commentat. Götting. Vol. II.

origin.

origin. It is true that Gmelin could, in his examination, employ no more than the fmall quantity of a few grains; but the refults were fufficient to fhew, that the colouring principle in his fpecimen originated not from cobalt but from iron.

A like refult is afforded by the following decompofition of the blue glafs from the ruins at Capri. and by our author.

Its colour is a *fapphire-blue* verging towards that of fmalt. It is tranfparent on the edges only. Its fracture, as well as that of the preceding, comes nearer to the fcoriaceous and conchoidal than to the fplintery. Antique blue glafs; almoft perfectly opake. Some of thefe blue glafs-plates are particularly diftinguifhed by this circumftance, that they are not coloured blue throughout the whole of their mafs, but only to about two-thirds of their thicknefs. Some plates are blue only to a certain depth. Each of the ftrata is fo nicely diftinct from the other, as to give the appearance of two plates adhering at their broad furfaces; the one blue, the other colourlefs.

(a) *Two hundred* grains of the above blue pafte were reduced to a fubtile powder, and fufed with 400 grains of *cauftic foda*. Fufion with foda, &c. gave filex. The obtained mafs, foftened with water, was faturated to excefs, and evaporated to a moderate drynefs. When rediffolved in boiling water, it depofited *filiceous earth*, which, after wafhing and ignition, amounted to 163 grains.

(b) The fluid was then fuperfaturated with *cauftic ammonia*. Alumine by the procefs with ammonia, &c. A brown precipitate thence enfued, which, upon edulcoration, I digefted with a folution of *cauftic potafh*. The flight portion taken up of it by this alkali was again, after faturating the lixivium with an excefs of muriatic acid, precipitated by means of *carbonated foda*, and proved, upon edulcoration and red heat, to be *aluminous earth*, amounting to three grains.

(c) What remained undiffolved by the cauftic potafh, was merely *oxided iron*, weighing nineteen grains when ignited and wafhed. Oxided iron.

(d) The liquor that had been fuperfaturated with cauftic ammonia and poffeffed a blueifh tinge, was by flow evaporation fo far reduced to a fmaller volume, that the greateft part of the muriate of foda, which had been generated and contained in it, could feparate in cryftals. The liquid contained a little copper and lime; but *no cobalt*. The fluid feparated from thefe, in which the acid predominated, and which now hardly exhibited any perceivable greenifh colour, was in vain examined

mined for cobalt *. It contained only a flight trace of copper and lime. The firft of thefe was made to appear, by combining the fluid with *pruffiate of potafh*, and the brown-red precipitates thus obtained amounted to a little more than two grains, which are to be confidered equivalent to about one grain of *oxided copper*.

(e) At laft, carbonated foda threw down about half a grain of *calcareous earth*.

Component parts of the blue pafte as enamel.

In confequence of this decompofition, thofe *two hundred* grains of the antique blue glafs-pafte, muft have contained the following earthy and metallic conftituent parts:

Silex - -	*(a)*	-	163.	grains.
Oxide of Iron	*(c)*	-	19.	——
Alumine - -	*(b)*	-	3.	——
Oxide of Copper	*(d)*	-	1.	——
Lime - -	*(e)*	-	0.5	——
			186.5	

Other experiments were made but fhewed no cobalt.

As I have fubjected the above blue glafs to feveral other experiments, merely with an intention of difcovering the cobaltic portion it might poffibly contain, yet without finding the leaft trace of it, there appears to be no doubt, that its blue colour entirely depends on the ingredient iron. That iron, under fome circumftances, is capable of producing a blue enamel, is clearly fhewn by the beautifully blue coloured fcoriæ of iron, which frequently are met with in the high furnaces on fmelting filiceous iron-ftones. But we are not fufficiently acquainted with the circumftances and conditions under which this colour is produced; for the affertion of Henckel, and fome other earlier authors, that by means of iron, cemented with arfenic, the fame blue tinge can be given glafs which it acquires from cobalt, has not yet been fufficiently

Iron can afford a blue enamel; as is feen in the fmelting works.

* It is well known that nature tinges the *fapphire, lapis-lazuli, blue clays*, &c. by means of *iron* without cobalt; but man is not poffeffed of her means. A chemical friend, with whom the tranflator had a converfation on this fubject and on the difficulty of proving the accuracy of the above *analyfis* by a *fynthetical procefs*, fuggefted the idea, that fuch a blue pafte could, perhaps, be made without cobalt by the intermedium of lapis-lazuli; an idea which may afford a fubject for experiment.—*Tranfl.*

confirmed.

confirmed. Whence, after the discovery of the blue from cobalt, the art of tingeing glass blue by means of iron has had the same fate with several other attainments now lost; namely, to have been discarded and forgotten on the account of new invented, more commodious, and certain expedients and methods.

SECOND SECTION.

The preceding glasses resemble our enamels;

THESE coloured musaic glass-pastes of the ancients agree, with respect to their opacity and scoriaceous fracture, with our modern enamels. On the other hand, the deceitful imitations of gems already mentioned before shew, that the ancients likewise knew how to prepare beautiful, high-coloured, and transparent glass-pastes.

but the ancients possessed another method of painting, little known to antiquaries:

But however well known those works in glass of the ancients may be, since both earlier and later writers have given sufficient information of them, and several specimens preserved in the collections of antiquaries afford a direct knowledge of this subject; it is, on the contrary, very surprising, that antiquaries are so little acquainted with that entirely peculiar and by far more remarkable painting on glass, which is formed of variously coloured delicate glass fibres, joined with the greatest nicety, and by subsequent fusion conglutinated into an homogeneous compact mass. In the earlier works on antiquities this scarce production of art is not at all mentioned; the reason of which is probably this, that the specimens now existing of it were found, perhaps, only about the middle of this *(last)* century.

It is formed of delicate fibres of glass united by fusion:

First mentioned by Count Caylus.

Among later antiquaries Count Caylus appears to be the first, who in his *Collections of Antiquities* has given information, accompanied by rather inadequate drawings, of this singular species of mosaic work. Winkelmann has afterwards, in his *Annotations on the History of the Art among the Ancients*, (page 5, *seq.*), more accurately described two other antiques of this kind, with the appellation, *Pictures made of Glass-Tubes*, in the following passage: " The works of the ancients in glass, which are not noticed in the *History of the Arts*, deserve particularly to be mentioned in this place; more especially, because the ancients carried the art of working in glass to a much higher degree than we have arrived at; a fact which, to those who have not seen their works of this kind, might

Ample description by Winkelmann.

might have the appearance of a groundlefs affertion." After which he mentions a floor formed of green glafs-plates difcovered in the Farnefe-ifland, as well as fome fragments of glafs-cups, which muft have been turned on the lathe, and then proceeds as follows: " But the art ftrongly claims our admiration in two fmall pieces of glafs, which laft year (1765) were brought to Rome. Each of them is not quite one inch long, and one-third of an inch broad. One plate exhibits, on a dark ground of variegated colours, a bird reprefenting a duck of various very lively colours, more fuitable to the Chinefe arbitrary tafte, than adapted to fhew the true tints of nature. The outlines are well decided and fharp, the colours beautiful and pure, and have a very ftriking and brilliant effect; becaufe the artift, according to the nature of the parts, has in fome employed an opake, and in others a tranfparent glafs. The moft delicate pencil of the miniature painter could not have traced more accurately and diftinctly, either the circle of the pupil of the eye, or the apparently fcaly feathers on the breaft and wings, behind the beginning of which this piece had been broken. But the admiration of the beholder is at the higheft pitch, when, by turning the glafs, he fees the fame bird on the reverfe, without perceiving any difference in the fmalleft points; whence we could not but conclude, that this picture is continued through the whole thicknefs of the fpecimen; and that, if the glafs were cut tranfverfely, the fame picture of the duck would be found repeated in the feveral flabs; a conclufion which was ftill farther confirmed by the tranfparent places of fome beautiful colours upon the eye and breaft that were obferved. The painting has on both fides a granular appearance, and feems to have been formed, in the manner of mufaic works, of fingle pieces; but fo accurately united, that a powerful magnifying-glafs was unable to difcover any junctures. This circumftance, and the continuation of the picture throughout the whole fubftance, rendered it extremely difficult to form any direct notion of the procefs or manner of performing fuch a work. And the conception of it might have long continued enigmatical, were it not that, on the fection of the fracture mentioned, lines are obfervable, of the fame colours which appear on the upper furface, that pervade the whole mafs from one fide to the other; whence it became a rational conclufion, that this kind of

Very curious antique enamel of a duck;

traced and finifhed with extreme accuracy and effect;

and continued through the whole thicknefs of the piece, minutely the fame on both furfaces!

It is found to confift of threads of glafs feen endwife.

of painting muſt have been executed by joining variouſly coloured filaments of glaſs, and ſubſequently fuſing of the ſame into one coherent body. The other ſpecimen is of about the ſame ſize, and made in the ſame manner. It exhibits ornamental drawings of green, white, and yellow colours, which are traced on a blue ground, and repreſent volutes, beads, and flowers, reſting on pyramidally converging lines. All theſe are very diſtinct and ſeparate, but ſo extremely ſmall that even a keen eye finds it difficult to purſue the ſubtle endings, thoſe in particular in which the volutes terminate. Notwithſtanding which, theſe ornaments paſs uninterruptedly through the whole thickneſs of the piece."

Another ſpecimen no leſs admirable.

Of the ſame glaſs-paſte, which has been here deſcribed, mention is made by Sulzer in his *Theory of the Polite Arts*, under the article *Muſaic* (moſaïch). Having ſeen the piece itſelf in the houſe of its then poſſeſſor, Caſanova, at Dreſden, he confirms, in the capacity of an eye-witneſs, the deſcription given by Winkelmann, and calls it "a remnant of antiquity, which indicates the exiſtence of an art brought to the higheſt degree of perfection."

The ſame were ſeen by Sulzer.

Mr. Townley, of London, enumerates, among the principal rarities of his celebrated cabinet of antiquities, the ſtone of a ring of a ſimilar antique glaſs-paſte, which repreſents a bird of ſo ſmall a delineation, that it cannot be diſtinctly ſeen but by means of a magnifying lens.

Extremely minute bird in Mr. Townley's collection.

As very few ſpecimens of this ſpecies of glaſs-painting, which undoubtedly muſt be reckoned among the loſt attainments of art, and of which even the exiſtence is ſtill ſo little known, are met with; I think it not ſuperfluous to give the following notice of two new ſamples which I poſſeſs of this claſs of antique ſubjects. Both pieces have a heart-ſhaped form. The principal front is flat, the reverſe is convex, and has from eight to ten prominences *(Ecken)*. The length of one of them is one inch, the breadth four-fifths, and the thickneſs two-fifths of an inch. The other ſpecimen is *two-thirds* ſmaller. As to colouring and manner of drawing, they are both nearly alike. The principal maſs of the *larger* is of a dark-blue, and wholly opake; but that of the *ſmaller* is a ſapphire-blue, and in ſome places tranſparent. The blue ground is embelliſhed with voluted, ſtellular, minute flowers, of ſo very ſmall a delineation as to be hardly imitable by the

Two ſpecimens in poſſeſſion of the author.

Deſcription.

pencil of the miniature painter. The colours of these flower-like ornaments, which are red, green, brown, sky-blue, and white, are pure and lively. The delineations pervade the whole substance, and upon a broken part it is seen by mere inspection, that these delicate figures have been formed of parallel glassy fibres of various colours, conglutinated by means of gentle fusion.

Coloured plate of the larger specimen.

As the drawings given by Count Caylus afford but a very imperfect idea of that ingenious enamel-painting, I submit here a delineation of the larger of my two specimens. *Fig.* 1, *Plate* XIII. represents it in its true size; *Fig.* 2 exhibits it magnified.

Invention of Meyer to multiply copies of paintings.

On this occasion the following passage from a letter of M. Käftner *, concerning an invention of the celebrated Tobias Mayer, may well deserve a place here. "Mayer possessed the art of making a number of perfectly similar copies of a painting. He compounded his picture of coloured *wax-crayons*, in the same manner as a prism may be composed of thinner prisms of the same length. Every transverse cutting afforded then a copy." Meyer might probably have been led to this method of imitation in wax, from the inspection of a musaic work of the kind here described.

II.

On Spontaneous Inflammations. By G. C. BARTHOLDI, *Professor of Physic and Chemistry.*

(Concluded from Page 220.)

4. *The Fermentation of Animal and Vegetable Substances.*

Heat from fermentation.

THE greater part of animal and vegetable substances, when they still retain humidity, and are accumulated into large masses, enter into fermentation, a change in their composition is effected, and they frequently heat to the point of inflammation. It is thus that magazines of hay, of turf, of flax, of hemp, stacks of hay or straw, heaps of linen-rags in paper-mills, &c. take fire spontaneously.

* *Allgemeine Geographische Ephemeriden*, by Zach, 1798.

It

It is principally hay which requires precaution; if the hay-harveft happen in rainy weather, it is commonly ftacked before it is thoroughly dry, and in this ftate is more difpofed to ferment and to heat. If a hay-ftack is obferved to be in fermentation, great care muft be taken not to throw it down too fuddenly, the exterior layers muft be carefully detached one after the other. It almoft always happens, that when a hole is made in the middle of a ftack of heated hay, it takes fire fuddenly.

Nothing, however, is more eafy than to prevent thefe fatal accidents: when there is any reafon to fear that the hay which is intended to be houfed or ftacked, is not fufficiently dry, it is only neceffary to fcatter a few handfulls of common falt (muriate of foda) between each layer. It would be very ill judged to regret this trifling expence; for the falt, by abforbing the humidity of the hay, not only prevents the fermentation and confequent inflammation of it, but it alfo adds a tafte to this forage which ftimulates the appetites of cattle, affifts their digeftion, and preferves them from many difeafes. Utility of adding falt to hay.

During the great heats of fummer, it frequently happens that heaps of dung inflame fpontaneoufly: great care fhould be taken to fprinkle them frequently with water in the fummer feafon, and to keep them at a certain diftance from habitations, as well to prevent fires as with a view to falubrity.

5. *The Accumulation of Animal and Vegetable Subftances covered with an Oil.*

If animal and vegetable fubftances heaped into a large mafs, can take fire from the heat produced by their decompofition, this accident is ftill more to be dreaded when they are covered with oily matters, and particularly with a drying oil. Heat from greafy animal and vegetable matters.

Befides the accident which happened at the manufactory of Lagelbart, and of which our colleague Hauffman gave an account to the Society, and the fire which took place in one of the fineft manufactories at St. Marie-aux-Mines, there are many other examples of wool, ftuffs, and pieces of cloth, which were not freed from greafe, taking fire in the warehoufes when they were folded together, and even while moving them from one place to another when they were in large quantities: this is principally to be dreaded when linfeed oil, or any

ny other oil, drying in itſelf, or rendered ſo by oxide of lead, is employed in the preparation of theſe ſtuffs.

In the manufacture of cloths, only olive oil, or oil of colza, ſhould be uſed to greaſe the wool.

It ſometimes happens on boiling flowers and herbs in oil, which occurs in ſeveral pharmaceutical operations, that, after being taken out, the herbs dried in the oil inflame ſpontaneouſly: care ſhould therefore be taken when theſe herbs are thrown away, not to heap them near other combuſtible bodies.

There have been ſeveral examples of veſſels having been burnt in ſea-ports, either by the ſpontaneous combuſtion of heaps of cordage coated with tar, or by a mixture of linſeed oil boiled with lamp-black, and incloſed in a bag.

6. *The boiling of oily Matters.*

Fire from boiling oily matters;

In the preparation of ſome varniſhes, ſuch as printers' ink, in which linſeed-oil, boiled to a certain conſiſtence, is generally made uſe of, it frequently happens that the oil takes fire, unleſs the neceſſary precautions are employed: the ſame effect takes place in melting butter, lard, or any other greaſe, if they are heated too much; ſo that, in theſe operations, it is always neceſſary to remove every other combuſtible ſubſtance, to have a lid at hand to cover the veſſel as ſoon as the fire has caught, and particularly to take care not to pour water upon it, which inſtead of extinguiſhing it, would ſpread it more and give it greater activity.

7. *Torrefaction.*

and from roaſted vegetable ſubſtances.

There are many vegetable ſubſtances which increaſe their power of inflaming ſpontaneouſly by torrefaction, if they are incloſed in ſacks of cloth, which leave them in contact with the ſurrounding air; ſuch are ſaw-duſt, roaſted coffee, the meal of grain, and leguminous fruits, ſuch as French beans, lentils, peaſe, *&c.*

There have been ſeveral inſtances of ſtables having taken fire from a bag of roaſted bran which had been applied to the neck of a ſick animal, and had inflamed ſpontaneouſly. The inhabitants of the country who, in ſome diſorders of their beaſts, are obſtinate in applying this remedy, to which others

of

of more efficacy and lefs danger might be fubftituted, fhould at leaft be careful not to inclofe the bran in the cloth, either too hot or too much roafted.

Brewers, after having made their barley and other grain, which they employ in making beer, germinate, dry it in a kiln, except what is intended for pale beer, and they generally roaft it more or lefs highly, to give the beer a deeper or paler colour. If, therefore, when the grain is brought from the kiln, it is put, ftill hot, into facks, it frequently happens that it heats and takes fire, and this has occafioned feveral fires in breweries.

8. *Sulphurated and phofphorated Hidrogen Gas.*

Inflammation of fulphurated and phofphorated hidrogen gas.

The caufe of fubterraneous fires and volcanoes is generally attributed to the decompofition of pyrites, or metallic fulphurets, buried in the interior of the earth. Thefe pyritous maffes are decompofed by the contact and concurrence of water and air, and the decompofition is always accompanied by a great expanfion of caloric, and a difengagement of a very inflammable gas, called fulphurated hidrogen gas. This gas inflames at an elevated temperature, and can communicate the inflammation to the fulphur of the pyrites, to the coal and other bituminous matters which generally accompany it.

Similar inflammations are fometimes obferved near coal-pits. In exploring the coal, veins and infulated maffes of pyrites are frequently met with: fince thefe pyrites always communicate a bad quality to the coal, the miners generally lay them afide, and throw them out of the pit: if thefe heaps of pyrites, mixed with coal, are then expofed to the alternate action of the fun and rain, they heat and inflame. Great care muft therefore be taken that thefe accumulations of pyrites fhould be kept at a diftance from all combuftible bodies to which they would neceffarily communicate the inflammation.

There are many operations of nature in which fulphurated hidrogen gas is formed, but it often enters into other combinations as it forms, it diffolves in water, or difengages at a temperature too low for it to inflame.

By boiling phofphorus in a folution of potafh or lime, phofphorated hidrogen gas is difengaged, which, being much more combuftible than fulphurated hidrogen gas, inflames at a low temperature as foon as it comes in contact with atmofpheric air.

This

This gas, which in chemical experiments offers the beautiful ſpectaçle of a fountain of fire over water, is produced naturally by the putrefaction of animal ſubſtances which are buried. The lights which are frequently ſeen to come out of the earth, and which are known by the name of Jack o' Lanterns, are only owing to the diſengagement of this phoſphorated hidrogen gas: as theſe lights generally appear moving about in places where they do not touch dry combuſtibles, they ſeldom occaſion diſagreeable accidents; but they are alſo diſengaged in foreſts, and it may happen that in hot ſummers, when the graſs and bruſh-wood are thoroughly dry, the gas in combuſtion may meet with theſe combuſtible materials, and ſet fire to them, and thus produce the conflagration of a whole foreſt: we ſhould not therefore, too lightly, and without ſufficient proofs, attribute to the malevolence or to the connivance of mankind, thoſe dreadful events which are ſometimes only the reſult of cauſes purely natural.

9. *Sulphuret and Phoſphuret of Lime and of Potaſh formed in the Combuſtion of ſeveral Vegetables.*

Sulphuret and phoſphuret of lime or potaſh.

Sulphur is always formed when gypſum (ſulphate of lime) or any other ſulphate, whether earthy or alkaline, is ſtrongly heated with charcoal, wood, or, generally, with any combuſtible which is reduced to charcoal by heat. The ſame ſalts form ſulphureous waters if the remains of animal or vegetable ſubſtances are left in a water in which it is diſſolved; ſo that it frequently requires only a little ſulphate of lime, or ſome other ſulphuric ſalt, to communicate the odour and taſte of ſulphur to water which is ſtagnant.

Pyrophorus.

Pyrophorus is obtained by calcining the alum of commerce, or ſulphate of potaſh with ſugar, meal, or any other ſubſtance which is reducible to charcoal.

The inflammation of pyrophorus which takes fire by the ſole contact of humid air, is only owing to the ſulphuret of potaſh, which by attracting the humidity of the air, heats to that point that it kindles the carbonaceous matter which ſurrounds it, and which being in a ſtate of greater tenuity, is ſo much the more diſpoſed to burn.

A pyrophoric matter may be formed in ordinary combuſtion.

But ſince many of our common combuſtibles contain ſulphuric ſalts, it may happen that, in their combuſtion, a pyrophoric matter is ſometimes formed by chance, which remains in the reſidue

refidue of the combuftion, efpecially if the combuftible is not entirely confumed, and a part is not reduced into charcoal, which frequently happens in the fire-places in which the combuftibles are not burned in grates, and the afhes are not feparated from the charcoal. There have been inftances of houfes having been burnt by afhes intermingled with the charcoal which had been taken too early from the fire-place and put into places where they were furrounded with combuftibles, which they fet on fire by a fpontaneous inflammation. Happily thefe caufes of conflagration rarely occur; for pyrophorus does not retain its property of inflaming for any length of time, and it is frequently decompofed fhortly after its production, without occafioning any unpleafant event: neverthelefs, care fhould always be taken not to put afhes newly burned, and which are ftill intermingled with charcoal, in places where they can communicate with combuftibles.

The formation of a pyrophoric matter is principally obferved in the preparation of the foda of commerce, which is obtained by the incineration of different maritime plants containing much fulphate of foda, and which, in the combuftion, fometimes furnifh a certain quantity of fulphur, greater or lefs, according to the manner in which the operation is directed.

The formation of phofphuret of lime has great analogy with that of fulphuret of lime. Although the phofphoric acid is not found fo often in vegetables as the fulphuric acid, it neverthelefs exifts in them in greater quantity than has hitherto been believed: it is principally found in moft plants which grow in marfhy places, in turf, and in feveral fpecies of the white woods. In reducing thefe woods into charcoal, a fmall quantity of phofphorus is fometimes formed, which may remain combined with the fame bafes as retained the phofphoric acid before the combuftion: the phofphorus, by forming other combinations, may be rendered incapable of occafioning any accident, but it may alfo happen from a concurrence of feveral circumftances, that charcoal impregnated with any phofphuret whatever, may, by expofure to the action of a warm and humid air, difengage phofphorated hidrogen gas, which, by the contact of the atmofpheric air, will take fire and communicate the inflammation to the mafs of the charcoal.

Phofphoric acid is abundant in vegetables.

Two examples of this kind of fpontaneous combuftion have taken place in the powder magazine of Effone, in the years 8 and

Spontaneous inflammation of charcoal in a powder magazine.

and 10. The firft time the receiver of the machine for fifting the charcoal caught fire, and the fecond time it began in the magazine of charcoal, without a fufpicion of any other caufe except that of a fpontaneous inflammation. The different reports made on thefe two events, have been inferted in the public journals; but the explanations that were given of them were not fatisfactory. It feems very probable that they were occafioned by fome phofphorus contained in the charcoal; and this explanation has the more weight, becaufe willow (bourdaine), which is ufed at Effone, as well as in moft other powder-manufactories, and which, in many refpects, deferves the preference over other woods in the preparation of powder, contains phofphoric acid, at leaft that does which grows in our neighbourhood.

Charcoal from turf begins to be employed in fome domeftic and other operations; but as it is much difpofed to fpontaneous inflammation, its ufe fhould be prevented, or at leaft it fhould be ftored with great precaution. It has happened at Paris and other places, that magazines of this charcoal, which were unfheltered, have taken fire by the combined action of the heat and rain.

10. *Phofphorus fometimes contained in Charcoal.*

Detonation from the phofphorus in charcoal.

It may alfo happen that the fmall quantity of phofphorus which is fometimes formed in the carbonization of different forts of wood, without uniting either with the lime or the potafh, remains combined with the charcoal, which then does not difengage phofphorated hidrogen gas, nor does it readily inflame by the fole action of water or of a humid air, but which, by percuffion with falt-petre (nitrate of potafh) may produce a powerful detonation. It is very probable that the three fucceffive explofions which took place in the powder-mill at the manufactory of Vonges, were partly owing to a fimilar caufe.

Charcoal probably one caufe of the bad quality of cold-fhort iron.

Charcoal has, in general, great influence on the different productions of nature and the arts. It is frequently obferved in forges and founderies, particularly in thofe of iron, that the products vary according to the nature of the charcoal employed. The bad quality which is fometimes found in iron, of being brittle when cold, is generally attributed to the phofphoric acid contained in the ores; but fince the fame ore, by the fame pro-

ceſſes, furniſhes better iron in one foundery than in another, the difference ſeems frequently to ariſe in part from the charcoal.

Such are the principal cauſes of ſpontaneous combuſtions, whoſe effects are ſo much the more dangerous by being leaſt expected. The Society of Emulation thought they ſhould render an important ſervice to every claſs of proprietors, and particularly to the inhabitants of the country, by developing the phyſical knowledge which might guard them againſt dangers, of which they are too frequently the victims, from ignorance and a fatal want of foreſight. I truſt I have fulfilled the wiſh of the Society, and of the firſt Magiſtrate, whoſe intelligence and conſtant ſolicitude extend, without exception, to every object which may contribute to the proſperity of the country and the happineſs of the governed.

III.

On the Solution of Water in the Atmoſphere; and on the Nature of atmoſpherical Air. By Mr. JOHN GOUGH. *From the Author.*

To Mr. NICHOLSON.

SIR,

I DO not recollect any philoſopher or meteorologiſt, who has attempted to demonſtrate the chemical union of atmoſpherical air and water, by help of the following facts and arguments. Should the preſent endeavour, to eſtabliſh the propoſition, appear deſerving of a place in your Journal, the inſertion of it will oblige,

Yours, &c.

JOHN GOUGH.

Middleſhaw, July 16, 1804.

Exp. 1. If a cylinder of dry porous wood be put into a ſtrong glaſs tube, nearly of the ſame diameter with itſelf, and water be poured into the veſſel, the particles of the fluid will penetrate the wood, and cauſe it to ſwell, ſo as to burſt the glaſs. Some writers affirm, that the ſame artifice has been uſed with ſucceſs to ſplit rocks, an operation which is commonly performed by the elaſtic power of gun-powder. A quantity of Dry vegetables attract water.

 motion

motion is generated in this experiment, which cannot be referred to the action of gravity. We must therefore ascribe it to another force, namely, the mutual attraction of wood and water in a liquid form.

Aqueous vapour attracted by dry vegetables, &c.

Exp. 2. If a piece of whip-cord or tharm be stretched by the heaviest weight, it can support, the pendent body will ascend, as oft as the string or gut contracts, in consequence of an accession of water derived from the atmosphere; on the contrary, it will descend, when the cord begins to relax from the loss of moisture. The motion generated in this instance, proves atmospherical vapour to be powerfully attracted by the dry fibres of vegetables and animals; consequently these substances have a strong affinity to water, not only in a liquid form, but also when it is diffused through the air. This affinity or force will be called the hygrometrical attraction in the sequel of the essay, for the sake of perspicuity.

The force of affinity permanent.

The preceding experiments have not the least claim to novelty; but they are the preliminaries of an inference, which is of moment in the present question. For affinity is a fixed relation of bodies, creating a disposition to coalesce, in such as are thus mutually related, as often as water is combined with another substance. The union must therefore be permanent, unless it happens to be dissolved by an external cause. Now as any certain force only gives way to another superior and contrary to itself, it is evident that a moist body, which discharges a portion of the water it contains, is obliged to part with it by a more powerful attraction, existing in its neighbourhood. It is to be remarked, that temperature is one of the external causes alluded to above; but it is disregarded at present, because the effects of its changes may be obviated in the following experiment, which is intended to throw additional light upon the hygrometrical attraction.

Hygrometrical attraction is diminished by the accession of water.

Exp. 3. Take two bibulous substances, such as two slices of sponge, or a piece of sponge and shred of woollen cloth. Make the one wet and keep the other dry; then put them both into a close vessel of glass or metal, placing them either in contact or apart: the wet body will grow lighter in a short time, and the dry one will gain more weight; this process may be prolonged, until the two substances find the equilibrium of their attractive powers; which will be accomplished, when their respective weights become stationary. This equilibrium proves the

the hygrometrical attraction to diminish with the absorption of water, and to increase with the loss of the same; consequently an union produced by this force, may be dissolved by the presence of a body, which contains less water, and therefore attracts it more powerfully.

Atmospherical air may be concluded to possess the power described above, from the changes and effects, which are observable in the following instances: First, Atmospherical air takes the water of crystallization from various salts; it therefore overcomes the affinity, which unites the component parts of these crystals. Secondly, The same hygrometer denotes a greater degree of humidity at one time, than at another, though the height of the thermometer be the same; consequently the hygrometrical attraction of the atmosphere is variable under equal degrees of temperature; because this force is evidently constant in an instrument kept in an uniform heat. Third, If two vessels be exposed, at the same time to the air, one of which contains dry potash, and the other a dilute solution of the same; the former will acquire weight, while the other grows lighter. The last fact shows, that atmospherical air may be saturated with moisture, in respect of one body, and be at the same time in a very different situation relative to another; so that evaporation evidently arises from an excess of hygrometrical attraction in the atmosphere; on the contrary, the production of dew depends upon a similar excess in the bodies on which it is formed.

Atmospherical air possesses this attraction.

I may be asked, after making this open declaration of my sentiments, which of the constituent gases of the atmosphere combine with water? The proper reply to the question appears to be this: It is atmospherical air; which I consider to be a homogeneous gas, for the following reasons:—First, The atmosphere is diaphanous; which could hardly be the case, were it a mass of uncombined fluids of different specific gravities; for, had such an arrangement been formed, the rays of the sun would have suffered a multiplicity of refractions, in their approach to the earth; and total darkness, or at best a dim twilight, would have been the consequence, had our planet been shrouded by a covering of heterogeneous gases. Thus the atmosphere appears to be homogeneous, from the consideration of its transparency.—Secondly, A given measure of oxigen is heavier than an equal bulk of azote, under similar

Atmospherical air a simple gas, because it is diaphanous, because smart reports are not double to sense.

Atmofpherical air a fimple gas.

circumftances; confequently the denfity of the former exceeds that of the latter, fuppofing their elaftic forces to be equal. On this account founds will move, in all cafes, with lefs celerity in oxigen than they do in azote. If then our atmofphere confifted of two independent maffes of thefe fluids, mutually pervading each other, every momentary report would have been double to fenfe, at a fufficient diftance from the feat of found; becaufe fuch a report would arrive at the ear more expeditioufly through the medium of the azote, than it would through that of the oxigen. But founds of the fhorteft duration are not repeated at the greateft diftances; confequently the air is homogeneous, becaufe it is the vehicle of found.

If the preceding arguments be juft, the homogeneity of atmofpherical air cannot be controverted; becaufe the conclufions which refult from the contrary hypothefis are repugnant to common experience. We come in the next place to the fpecific nature of this gas; but this is a difficult enquiry in the prefent unfettled ftate of chemiftry, when the phenomena of galvanifm are daily bringing new truths to light, and threaten to fubvert the prevailing theory. Conjectures, however, will naturally fpring up in the midft of uncertainty; and as a diverfity of fentiment has its ufe in times of fcientific anarchy, I will venture to propofe the following hypothetical queftions relative to the conftitution of common air. Is not this fluid a chemical compound, having the gas called azote for its bafis; to which the pofitive energy of the galvanic pile is united, together with water, but in a manner which diftinguifhes this compound from the gafeous oxide of azote? May not a gas thus conftituted, oxidate other fubftances through the interpofition of the water, which it holds in folution by the hygrometrical attraction? Though the aqueous part of the atmofphere cannot of itfelf decompound common air; may not it perform the office of an intermediate agent, when affifted by the body to be oxidated, and in this manner deprive the azote of the galvanic energy, more or lefs perfectly, according to circumftances? Will not the aqueous vapour unite with the matter feparated from the air, and produce oxigen gas, which will enter into compofition with the third fubftance, and complete the bufinefs of oxidation? The hints fuggefted in the preceding queries, would have been by no means admiffible in a time of more perfect uniformity in the fentiments of philofophers;

losophers; and nothing can be pleaded in its excuse but the revolution of opinion, which is apparently ready to take place in the theory of gaseous fluids.

IV.

Reply to the Observations of M. l'Abbé Hauy, on arseniated Copper. By M. le Comte de Bournon, *Member of the Royal and Linnean Societies of London.**

Introductory remarks.

It is but a few days, Sir, since I had the honour to receive from you the observations which you have made on the different species of arseniated copper, described by me, in a memoir on that subject, read to the Royal Society of London, on the 19th of February, 1801. I have read these observations with the greatest interest, but not being able to adopt the opinion respecting them, to which the investigations you have submitted them have led you, I feel great obligation for the opportunity you have afforded me of explaining myself more particularly than I have hitherto done, on what relates to this interesting subject. Besides, you offer these observations with that diffidence which usually characterizes real merit, accompanied with a doubt which calls for a new examination.

You oppose my opinion, Sir, on the division which I have made of the arseniated copper into four species, with a delicacy and a politeness, which renders the slight mineralogical discussion that becomes the necessary result of it, of infinite value to me. It is very desirable that those facts on which differences of opinion may prevail should always be discussed in this manner: the sciences would certainly gain by it, and those who cultivate them would lose nothing by yielding a little to each other.

Notice of the former enquiries.

Like you, Sir, when I employed myself on the substance which, since the first essays of the celebrated Klaproth on it, has been called a combination of the arsenical acid and copper, I thought it right to consider, under the same point of view, the different crystalline forms which it offered, deriving them all from one common base, and my first enquiries were

* Translated from the original, communicated by the author. For the paper of the Abbé, see p. 187 of our present vol.

directed

directed to determining this base, or the primitive generating crystal of all those of this substance. I was not long in discovering that among the crystals which I had subjected to this examination, there existed two forms which could not, in any way, be connected with the others: analysis has since shown, that one of these belonged to an arseniated iron, which had been improperly cited as belonging to copper, and the other to a combination of copper and iron with the arsenical acid, which had not been known before. With respect to the other crystals, as the appearance offered by each of them contradicted the opinion which connected them, it became necessary to depend in the best possible manner on all the other exterior characters which this substance could offer to the mineralogist, to attain to some result respecting it. This is precisely what I have done, and when the aggregate of these characters forced me to recognize four very distinct species in the mass of substances, which I suspected might belong to the combination of the arsenical acid and copper, I confess I experienced some satisfaction in observing that the analysis of a chemist, so justly esteemed as Mr. Chenevix is, sanctioned, in some measure, the division to which observation had led me. You remark, Sir, that these analysis, on being repeated by M. Vauquelin, varied in their result: it follows necessarily, that this support fails, or at least becomes uncertain for me: I abandon it therefore, and leave to chemistry the discussion of a fact which belongs to it, and was to me only a powerful auxiliary, to confine myself within the strict limits of mineralogy properly so called.

Preliminary observations on the mode of conducting mineralogical enquiries.

But allow me, Sir, first to make some previous observations on the method which, it appears to me, should be followed to determine the union or separation of substances, and afterwards, on the possibility of finding several species placed under the combination of the same acid with the same base, but without doubt, having essential differences in the manner of combination.

The methods to be employed by the mineralogist in the study of mineral substances, are comprized in the examination of the peculiar marks which nature has impressed on each of the individuals which decorate and enrich its bosom, and which his great habit of observing has taught him to recognize. Of these marks, which we designate by the expression of exterior specific

cific characters, some are too delicate to be described; but custom enables the naturalist to seize them; their action on his sight is sudden; the most rapid glance embraces the whole of them, and the naturalist has frequently formed his opinion long before he has thought of accounting to himself for it. He is not, however, secured by them from the errors which other bulkier and more comparable characters may afterwards rectify; but the first impression received from these first traces, very often serves him as a guide in the method of employing the second. Among these latter characters, some are of easy application and almost always possible, others require attention and particular circumstances to be capable of being employed. Those which are in most common use, and easiest, are the form, the fracture, the hardness, the specific gravity, and the colour. Perhaps in a skilful hand, directed by the habit acquired from their use, these characters are almost always sufficient for the knowledge and classification of mineral substances. In stones, the colour is the most variable of all: nevertheless, it is certain, though the true cause cannot yet be assigned, that each of those which have been examined hitherto, affects one only of the known colours more readily than it does any of the others. But in the metals this character becomes more constant and more essential, and it very seldom varies without the cause of its variation being a change in the nature of the metallic substance itself.

This fact granted, when the naturalist employs the exterior specific characters, to ascertain the subject which determines his enquiry, from the moment at which the agreement of these characters, or their differences with those shown by known substances, puts him in a situation to pronounce on the identity or the difference of their nature, do not you believe that he has then the liberty of retrenching, on the one hand, those which do not agree with the opinion which he had previously thought it right to embrace; and, in the second place, to subject the others to suppositions which may occasion a change in their aspect to connect them with that which he wishes, when nature itself has not offered traces, free from doubt, of the probability of the modification which he admits in these characters?

The consideration of some of the characters may be omitted when they do not lead to general inferences.

Permit me to observe to you, Sir, that this is precisely what appears to me to be the substance of your observations on the arseniated

This has been done by M. Hauy.

arfeniated copper. You feem to confider as nothing the very fenfible differences which exift in the divers fpecies which I have eftablifhed, with refpect to hardnefs, fpecific gravity, and colour; and, ftopping at the fingle character of form, you make fuppofitions for each of them, which, in fact, terminate by connecting all thofe which they offer with one primitive cryftal: but nature does not exhibit any of the decrements which you fuppofe. I have never difcovered the flighteft trace of them in any of the immenfe quantity of cryftals of arfeniated copper which have paffed through my hands. Do you believe that thefe fuppofitions are only fufceptible of being admitted in a cafe in which, all the other characters being agreed in the moft perfect ftate of thefe fubftances, which is that of regular cryftallization and tranfparency, they would become neceffary only to add an accumulation of proofs to thofe already acquired of their identity.

Exact diftinction of the fpecies neceffary to the progrefs of the fcience.

Never was more attention paid than at this moment to the great truth, that the progrefs of the fciences which lead to the ftudy of nature, depends principally on the exact diftinction of each of the fpecies whofe union forms the aggregate to which the fcience is applied. No one is more convinced of this important truth than I am. But this exact knowledge of the fpecies, which perhaps your calculation or the analyfis of improved chemiftry may one day attain in a fimple and accurate manner, refts at prefent on the agreement of the exterior fpecific characters. Whenever this agreement exifts, we are compelled to conclude that there is a fimilitude in the fpecies, and, on the contrary, a diffimilarity when they differ effentially from each other. I, however, agree perfectly with you, that before feparating one of thefe fubftances from the other to make a fpecies of each, it is requifite to be previoufly convinced that the differences which they offer, and on which their divifion refts, are not purely accidental. It appears to me, therefore, that nothing can be more undeferving of the reproach of having neglected thefe precautions, than, on the contrary, the eftablifhment of the divifion on the invariable conftancy in the difference of their exterior characters.

Chemical analyfis does not fuperfede the mineralogical characters.

The only reafon, which, in the fubftances in queftion, can raife any doubt on their difference, is the refult obtained from them by chemical analyfis, which conftantly found the arfenical acid combined with the copper in each of them; but if the

analyfis

analysis had not been possible, certainly no naturalist would have hesitated to separate them from each other, according to the exterior characters shown by each of them.

Why therefore, because these substances all belong to the combination of the same acid with the same metal, should there not be found several species among them? This, I believe, is a fact which occurs much more frequently than has been hitherto supposed. Do not all the metals show various instances of striking differences in the oxides, in consequence of that which exists in the combination of oxigen with them? The octahedral attractive oxided iron, that which is rhomboidal, that not attractive, are not all these so many species? In a memoir which was inserted in the 75th number of the *Journal des Mines*, I have endeavoured to show that the octahedral sulphurated iron, and that in cubes, formed two very distinct species, and I do not believe that these are the only ones which exist in it. How many species are offered by sulphurated copper! I am myself acquainted with six, all perfectly distinct and characterized, which I have long intended to give the description of, but my occupations and want of time have not yet permitted me. Finally, have not you yourself been compelled to form a particular species of carbonated lime, of the arragonite, from the difference alone, which exists in its exterior specific characters, although chemistry can only find carbonic acid and lime in it?

Variations in the species of the same chemical combination.

One reason which may be alledged against the division of arseniated copper into species, is that the combination of copper with the arsenical acid being already a species in the genus of ores of copper, it would be making species of a species: and this objection, which at the first blush appears well founded, would bear equally against the various oxides, sulphurets, &c. But this difficulty seems to me to be more specious than solid: it takes its rise from the impossibility in which we still are of ascertaining every thing connected with the different causes which may produce a variation of the species. Without doubt, in this instance, for example, it is not the simple combination of the arsenical acid with the copper which forms the species, but the particular combination of this acid with the metal. Thus it is not the simple combination of oxigen, hidrogen, carbon, azote, &c. which constitutes the particular species of animal, but the manner of the combination itself.

The

Mineralogy is not yet ſufficiently perfect to admit of the ſeparation of the ſpecies by the form of the primitive moleculæ alone.

The mineralogical ſpecies are very accurately determined by the agreement or difference which exiſts in the firſt moleculæ of the formation of the different ſubſtances, but until we have fixed data to appreciate, in a determinate and invariable manner, all which relates to theſe moleculæ, the conſtancy or the difference in theſe exterior ſpecific characters will always be the only means within our power of uniting or ſeparating the ſpecies. I acknowledge, however, that in this caſe, it is neceſſary that this diviſion ſhould be eſtabliſhed as much as poſſible on ſtriking and eſſential characters; and I agree, at the ſame time, that latterly, this method has perhaps been much abuſed by giving importance to ſimple and caſual characters; this has frequently placed ſubſtances in the number of ſpecies, which ſhould only have been conſidered as ſimple varieties of thoſe already known.

I ſhall now beg of you, Sir, to compare with me the different ſpecies of arſeniated copper which I have deſcribed.

Compariſon of the Firſt and Second Species.

Compariſon of the firſt and ſecond ſpecies of arſeniated copper.

The form of the firſt ſpecies is an obtuſe rectangular octahedron, whoſe faces are unequally inclined. Two of them meet at the ſummit under an angle of 139° and at the baſe under one of 50°. The two others meet at the ſummit in an angle of 1,15°, and at the baſe in one of 65°. This octahedron is uſually cuneiform: I have never perceived any modification of it.

The form of the ſecond ſpecies is a hexahedral plate, always very thin, whoſe vertical planes are inclined alternately in oppoſite directions, ſo that two of them, on the ſame ſide, make an angle of 135° with the terminal faces to which they incline, and the third, one of 115°.

The moſt uſual colour of the firſt ſpecies is a deep and very brilliant ſky-blue, which ſometimes changes to green.

That of the ſecond ſpecies is a fine emerald green: I have never ſeen any other.

The ſpecific gravity of the firſt ſpecies is 2881: that of the ſecond 2548.

The hardneſs of the firſt is ſuch that it readily cuts carbonated lime: the ſecond is no harder than is ſufficient to cut gypſum.

In

In your obfervations, Sir, you have raifed to 50° 4′ and 65° 8′ the meafures which I had eftablifhed at 50° and 65°; meafures which you have fixed from the relations eftablifhed by you between the height of one of the pyramids and the perpendiculars drawn from its bottom on the edges of its bafe, which correfponds with the adjacent and unequally inclined, pyramidal faces. Thefe meafures are fo near to mine, that I have proved them again, and the inftrument is fo little capable of marking this difference, that I do not make any difficulty in adopting them.

To connect the form of the fecond fpecies with the obtufe octahedron of the firft, you afterwards fuppofe two fections made parallel to one of the moft inclined faces of the octahedron, fo as to detach a very thin fegment, in which the centre of this octahedron is to be contained; that is to fay, this octahedron is to be confiderably increafed on all its faces, with the exception of one alone, taken on each pyramid, and in an oppofite direction in each of them. You fuppofe at the fame time a decrement of a fingle row along the edges of the bafe, but which acts only on two of the faces of the octahedron, and that the fegment which refults on two of the three fides inclined to each of the terminal faces, makes with them an angle of 130½° and the third 115°, meafures which only differ 5° 30′ in the angle of 130° 30′ from thofe which I have given for the cryftal.

The following is the anfwer dictated by the new examination which I have made of this fubftance.

Agreeably to what I have faid in my memoir on the arfeniated coppers, the obtufe octahedron frequently fhows flight ftreaks in its faces parallel to its edges, which indicates a lamellated texture in the direction of thefe faces. The fracture alfo indicates the fame texture, but its fractures are always more or lefs irregular. I have never been able to obtain a clear one. In the fecond fpecies, on the contrary, the laminae are as eafily raifed from the hexahedral terminal faces, as could have been done on a prifm of mica. Thefe terminal faces are fometimes ftreaked parallel to the edges of the fides which are inclined to them, and thefe ftreaks, which are continued ftrongly on the fides, never appear upon them except in this direction. This texture, very analogous to that of mica, feems to me to be totally different from that of the obtufe octahedron of the firft fpecies. Objections to their feparation refuted.

I have

I have submitted some new crystals of this species to measurement, and have found them agree perfectly in the measures of 115° and 135° with those which I had examined before. The angle supposed by you of 130° 30′, which I have tried on a number of crystals always appeared to me to be much too small. These crystals have offered me a new variety, in which the sides of the hexahedral laminæ are less inclined on the terminal faces, with which they form an angle of about 105°. The crystal which afforded me this new variety is four lines in diameter: it is only in perfect preservation in one of its halves, but it admits of a judgment from it, that all its sides must have the same inclination. These new faces are perfectly smooth, and do not show any striæ. On another crystal, instead of these inclined sides, two planes are observed, one of which belongs to that which made an angle of 105° with the terminal face to which it inclines, and the other belongs either to that of 115° or that of 135°.

A new variety of the second species.

There was not any thing that I could discover connected with any of the planes of the obtuse octahedron of the first species.

Comparison of the Third Species with the First.

Comparison of the third species with the first.

The colour of the first is either a deep sky-blue or a grass-green. That most usual in the third is a yellowish green, more or less deep, but very frequently it can only be perceived by placing the crystal between the eye and the light, the intensity of the colour making the crystals appear black in every other position.

The specific gravity of the first is 2381; that of the third 4280.

The hardness of the first is not more than sufficient to scratch carbonated lime; that of the third is such as to cut fluated lime.

The first species has an obtuse rectangular octahedron for a unique and primitive crystal, whose dimensions have been given above: the figure of the third is an acute rectangular octahedron, in which each pyramid has two faces more inclined than the other two; the two most inclined faces meet at the summit, in an angle of 84°, and at the base in one of 96°, and the two others meet at the summit in an angle of 68°, and at the base in one of 102°. This octahedron is most usually cuneiform, and its prolongation is sometimes very considerable;

derable; it then takes the appearance of a rhomboidal tetrahedral prifm of 84° and 96°, terminated at its extremities by a dihedral fummit with ifofceles triangular planes, the fummit of which is placed on the edges of 84°, and the bafes meet with each other in an angle of 112°. This form has not hitherto fhown any other modification except being replaced by a plane, larger or fmaller, on the edges of 96°. Its planes are ufually very fmooth and brilliant, and I have never been able to difcover an appearance of divifion *(clivage)* in any of them.

This third fpecies paffes by the greatly lengthened octahedron to the determinate capillary variety, as well as to that which is indeterminate, and in this cafe the colour appears either to tend more to green or to take a more defined yellow, which fometimes has the brilliancy of gold.

The firft fpecies does not exhibit any thing which refembles thefe various tranfitions; it is always the fame obtufe octahedron, and only varies by a very flight prolongation of its cryftals parallel to the leaft inclined faces. To make the formation of the acute octahedron of the third fpecies, fecondary to the obtufe one of the firft, you fuppofe a decrement at the bafe of the latter of two rows above and below the edges of the union of the leaft inclined faces, and another of four rows at that of the union of the moft inclined faces, and by this, you get an acute octahedron, whofe moft inclined faces meet at the fummit, in an angle of 86° 24′ and at the bafe in an angle of 93° 36′; and the others meet at the fummit in an angle of 71°, and at the bafe in one of 109°.

I acknowledge that this approximation to the meafures which I have given is feducing, and, confidering the natural fmallnefs of the cryftals of this fpecies, it would perhaps be difficult for me to pronounce determinately whether the meafures which I have taken are much more exact than thofe to which you have attained by calculation; but this I can affure you, that no indication whatever, in either of thefe two octahedra, leads to the fuppofition which has given you this refult.

From the details I have now given, it is eafy to deduce the reafons which impel me to adhere to the divifion which I have thought it right to make in the arfeniated copper, and prevent me from adopting the approximation to which your ingenious hypothefes have led you. Every thing ftill feems to me to tend

Reafons for adhering to this divifion of the fpecies.

to indicate a difference in the ſpecies into which I have ſeparated them, while to bring them to a ſingle one, you have been obliged to conſider as nothing, all the exterior ſpecific characters, with the exception of the form alone; and you have uſed the latter only in eſtabliſhing an hypotheſis reſpecting it, to which, neither artificial means, ſuch as ſplitting, nor natural indications, ſuch as ſecondary planes on a primitive cryſtal, nor the retaining of primitive planes on the others have led you. If in the explanation of embaraſſing facts, it were permitted thus to make nature ſpeak when ſhe is ſilent, I have no heſitation in aſſerting that a naturaliſt ſo well informed, and ſo practiſed in the art of calculation as you are, would find very few obſtacles in reſolving all the ſpecies into each other whenever he choſe.

Of the four ſpecies of arſeniated copper which I have deſcribed, there ſtill remains one, in reſpect of which you have not made any calculation of approximation; it is the fourth, which, as I have ſtated, has for a primitive cryſtal a tetrahedral priſm with an equilateral triangle for a baſe. Nevertheleſs you do not exclude it when you draw your concluſions on the doubt which you believe to exiſt on the diviſion of the arſeniated coppers into four ſpecies, and you direct this doubt equally to the fourth. Since according to your ſuppoſition this can only be as a primitive to the firſt cryſtal, that it may alſo be in a ſtate to be brought to it, I have thought it right to add likewiſe the compariſon of this fourth ſpecies with the firſt.

Compariſon of the Fourth Species with the Firſt.

Compariſon of the fourth ſpecies with the firſt.

The colour of the firſt ſpecies is a deep ſky-blue, which ſometimes changes to graſs-green. That of the fourth is a brilliant deep verdigris, but its ſurface is very readily diſcoloured, doubtleſs by oxidating, and it then becomes black: this renders the cryſtals opaque, which, when they have not experienced this alteration, are beautifully tranſparent; this is very unuſual among thoſe which have been naturally expoſed to the free air during a certain time. This change however exiſts only at the ſurface; by ſcratching the cryſtals lightly, their fine colour is readily reſtored to them. I have never perceived any thing reſembling this fact, which unqueſtionably depends on the nature of the ſubſtance of this ſpecies, either among the cryſtals of the firſt, or among thoſe of the ſecond and third.

The

The specific gravity of the fourth species is 4280, and is consequently perfectly analogous to that of the arseniated copper of the third species, but at the same time greatly inferior to that of the first, which is 2881.

Its hardness, much below that of the third species which we have just seen that it resembles in weight, is also less than that of the first species, by which it is cut.

Its forms, which are greatly multiplied, while there exists but one in the first species, differ essentially from those of the first.

All these forms appear to me to be derived from the right tetrahedral prism with equilateral triangles for bases; and all those which I have endeavoured to recognize and have given in my memoir, seemed to me to be very readily derived from that, as I have also said: these crystals are always extremely small, and I was unable to measure them; a few groups of them exhibited this prism in such a manner that I could observe it perfectly complete. The scarcest crystals, after those belonging to the varieties which I have represented at figures* 15, 16, and 17, and which are usually so grouped as to penetrate each other, and thus to become very difficult to be known, are the very acute complete rhomboid and its incomplete varieties, such as are represented at figures 22, 23 and 24 of my memoir. I even hesitated, the division *(clivage)* not having shown any thing to guide me, whether I should not take this rhomboid for the primitive form. In this instance, the only manner which seemed to me natural and simple of connecting this crystal with the obtuse octahedron of the third species, is to suppose this octahedron become rhomboidal by an increase having taken place by the superposition of laminæ, or by the collection of rows of moleculæ, growing smaller, on only one of the faces of each pyramid, and in an opposite direction on each of them, as occurs in the spinel, and a number of substances having a right octahedron for a primitive crystal, and which I have also seen in the diamond. But in this case, either the increase must be made on the most inclined faces, and then calculation shows that the planes of the rhomboid should have 57° 39′ and 122° 21′ for the measure of the angles of its plane; or the same increase must be made on the least inclined faces, and then the measures of the angles of the plane of the rhomboid

Reasons for forming a fifth species.

* Phil. Transf.

Reasons for forming a fifth species.

should be 47° 4′ and 132° 56′: now although from its minuteness, the rhomboid which exists in this species is not measurable by instruments, yet I can affirm with certainty that it is much more acute than either of the two which this supposition gives rise to.

It is very true, Sir, that since my memoir on the arseniated copper was printed, I have thought it right to separate the species described in it, and to make a fifth of one of the substances included among them; a substance which is entirely different from the others in its exterior characters, and seems to lead naturally to a belief that water must be a principal in the number of its component parts: but you are in an error with respect to which of these substances I believe to be really of a different nature from the others. In my first work on the arseniated copper, I found it necessary to make several subdivisions or varieties in the fourth species: of these, the three first are the determinate capillary, the indeterminate capillary, and that which is solid at one of its extremities, and divided into very delicate fibres at the other. You seem to believe that I comprehend these three varieties in the new species which I have been led to consider as an hidro-arseniate. This opinion would indeed be, as you very justly observe, completely contradictory of every thing which I have said on these varieties, in the description I have given of them, and I have no doubt, must have astonished you. The only species of arseniated copper to which this opinion is directed, is that which includes the two varieties to which I have given the names of hematiform and amiantiform: they are very certainly the same, with this difference, that one is the product of the decomposition of the other.

This arseniated copper, when it is unmixed, forms very compact mamellæ, but striated from the centre to the circumference, and very often also forming different concentric layers: their colour is brown, sometimes with a very slight tinge of green. This substance, in its aspect, greatly resembles the hematiform oxide of tin, which, in *Cornwall*, bears the name of *wood-tin*; this has caused the miners of the same country to give the name of *wood-copper* to this species of arseniated copper. Its hardness, notwithstanding its fibrous texture, is sufficiently great to scratch fluated lime with facility. Its specific gravity is from 4100 to 4200.

This

This substance very readily changes; it then passes to an ash-grey, and loses considerably of its hardness: it also frequently experiences a more advanced decomposition, and then becomes perfectly white; and so soft that the nail is sufficient to cut it and separate its fibres. If the mamellæ which have passed to this state are broken, their decomposition is frequently found to have reached their centre; but very frequently also the centre has perfectly retained its brown colour and its hardness, and both are observed to diminish gradually on approaching the circumference; if in this case the attention is directed to the fibres near the circumference, it will be seen that they are detached from each other, and that the surface itself of these mamellæ has the appearance of that of madrepore, from the immense number of fissures in different directions, which are occasioned by the contraction. At length this substance arrives at such a degree of decomposition, that the mamellæ open completely, their fibres are entirely separated from each other, and, in this state, frequently become so slender and flexible as perfectly to resemble small portions of papyraceous amianthus. Reasons for forming a fifth species.

Such, Sir, is the nature of the arseniated copper, in which I believe I have found properties and a mode of existence which differ from those of the others, and which I have had opportunities of examining with greater facility and accuracy since the impression of my memoir. This easy decomposition, the prodigious contraction, and the great change which this substance experiences in itself, have led me to suppose that the loss of water has great influence on it. But this fact is, however, nothing more than an opinion, which experience will either reject or confirm; and I have so stated it. It may also be very possible that this arseniate is no more than a variety of the third species, as I at first considered it. You must, however, acknowledge that it offers very singular characters, of which it would, therefore, be interesting to know the cause.

The dealers in minerals from London, and principally Mr. Mawe, have, I believe, taken a collection of arseniated copper to Paris. This substance appears to me worthy of engaging the attention of chemists, who, I am of opinion, should repeat the analyses of it. Perhaps they may one day throw a clearer light on a substance which has greatly interested me, and to which I am at this moment indebted for the pleasure of having entered with you, Sir, into a discussion, agreeable in its manner and instructive in its effect.

V.

Process for separating Alumine from Alum. In answer to the Enquiries of R. T. *By Mr.* Frederick Accum.

To Mr. NICHOLSON.

SIR,

IF you have not received a better answer to the queries of your correspondent R.T. (p. 141) who finds so much difficulty in obtaining alumine in a state of purity, &c. I request you will give a place to the following lines in your next. I have endeavoured to be as concise as possible, and have the honour to be,

Sir,

Your most obedient humble servant,

FREDERICK ACCUM.

Old Compton-Street, Soho,
July 17, 1804.

Common process for separating alumine from alum, throws down a salt, and not a mere earth.

The process in general recommended by systematic writers for obtaining alumine, (which R. T. adhered to) is absolutely erroneous, for no pure earth of alum can be expected if a solution of alum of commerce be decomposed by a solution of carbonated alcali, and subsequent ablution and expulsion of the carbonic acid. For alum of commerce is a triple compound, consisting of alumine, potash and sulphuric acid in excess. If we attempt to saturate this excess of acid, by the addition of an alcali, or even by pure alumine, a highly insoluble salt, (sulphate of alumine) is generated, differing from alum principally in the proportion of its constituent parts. When we therefore gradually add (as R. T. did) to a solution of alum, a carbonated alcali, the first effect of it is, to saturate the excess of the sulphuric acid, and the precipitate consists principally of the salt, which is insoluble in water. A farther addition effects instantly a decomposition of part of the salt, which, in proportion as it takes place, becomes mixed with the alumine, which is thus covered or defended from the further action of the alcali. This being the case, it is obvious that no subsequent washing can do more than separate the sulphate of potash, and therefore the residuum, instead of being pure alumine (as R. T. imagines) contains also a variable portion of true sulphate of alumine.

To

To prove what has been ſtated before, let any quantity of the obtained alumine be heated in contact with charcoal powder; introduce the mixture into a tabulated retort, connected with the pneumatic apparatus, and add to it muriatic or ſulphuric acid; the reſult will be ſulphuretted hidrogen gas in abundance, particularly if heat be applied to the mixture. The production of this gas will become evident on conſidering the philoſophy of the proceſs. By means of the following method, alumine may be obtained in a purer ſtate.

The precipitate or ſuppoſed earth, if heated with charcoal, and then treated with an acid, gives ſulphuretted hidrogen.

Take any quantity of alum of commerce, diſſolve it in four parts of boiling diſtilled water, and mingle this ſolution with liquid ammonia till no further cloudineſs enſues. Then heat the mixture nearly to the boiling point for a few minutes, and transfer it on a filter. In proportion as the fluid paſſes off, pour more water over the precipitate, and continue the ablution till the water runs off taſteleſs. Having done this, let the precipitate while yet in a paſty ſtate, be transferred into a baſon or flaſk, and add to it muriatic acid, in ſmall quantities at a time, until the whole is diſſolved; then evaporate the ſolution till a drop of it, when ſuffered to cool on a plate of glaſs, yields minute cryſtals. If it now be ſuffered to cool, cryſtals of alum will be depoſited. Remove theſe cryſtals, by decanting the fluid, and renew the evaporation, until on further cooling, no more cryſtals are formed. Nothing now but nearly pure alumine remains in ſolution; the potaſh and ſulphuric acid being got rid of, at the expence of a little alumine in the cryſtals. The fluid may therefore be diluted with water, and then decompoſed by liquid ammonia, taking care to add this alcali in exceſs. The precipitate thus obtained, when waſhed and dried, will be alumine in a ſtate of conſiderable purity.

Method of ſeparating purer alumine.

Precipitate by ammonia; and waſh well. Diſſolve in muriatic acid. Evaporate and cryſtallize for a contaminating portion of alum. Then dilute and precipitate again by ammonia.

The alumine thus obtained does *not* yield ſulphuretted hidrogen when heated with charcoal power; it emits *no* odour when breathed upon, it is ſomewhat unctuous to the touch, inſipid, and white as ſnow.

The earth that now falls is conſiderably pure.

Short

VI.

Short Account of Mr. Arthur Woolf's Improvement in the Construction of Steam-Engines.

Discovery that heated steam may be suffered to expand, with a very powerful effect;—

MR. WOOLF founds his improvements on an important discovery he has made respecting the expansibility of steam when increased in temperature beyond the boiling point, or 212° of Fahrenheit's thermometer. It has been ascertained for some time by Mr. Watt, that steam acting with the expansive force of four pounds per square inch against a safety-valve exposed to the atmosphere, is capable of expanding itself to four times the volume it then occupies, and still be equal to the pressure of the atmosphere. Mr. Woolf has discovered and established by proof, that steam of the force of five pounds the square inch, may expand itself to five times its volume; that masses or quantities of steam of the like expansive force of six, seven, eight, nine, or ten pounds the square inch, may be suffered to expand to six, seven, eight, nine, or ten times its volume, and will still be respectively equal to the atmosphere, or capable of producing a sufficient action against the piston of a steam-engine to cause the same to rise in the old engine (with a counterpoise) of Newcomen, or to be carried into the vacuous part of the cylinder in the improved engines first brought into effect by Messrs. Boulton and Watt;—that this ratio is progressive, and nearly if not quite uniform, so that steam of the expansive force of 20, 30, 40, or 50 pounds the square inch of a common safety-valve will expand itself to 20, 30, 40, or 50 times its volume with like effect; and that, generally, as to all the intermediate or higher degrees of elastic force, the number of times which steam of any temperature and force can expand itself is nearly the same as the number of pounds it is able to sustain on a square inch exposed to the common atmospheric pressure: provided always that the space, or vessel in which it is allowed to expand itself, be of the same temperature as that of the steam before it was allowed room to expand.

—the increase of temperature not being considerable.

Respecting the different degrees of temperature required to bring steam to, and maintain it at, different expansive forces above the weight of the atmosphere, Mr. Woolf has

found,

found, by actual experiment, ſetting out from the boiling point of water, or 212°, at which degree ſteam of water is only equal to the preſſure of the atmoſphere, that in order to give it an increaſed elaſtic force equal to five pounds the ſquare inch, the temperature muſt be raiſed to about $227\frac{1}{2}$°, when it will have acquired a power to expand itſelf to five times its volume, ſtill being equal to the atmoſphere, and capable of being applied as ſuch in the working of ſteam-engines, according to his invention: and with regard to various other preſſures, temperatures, and expanſive forces of ſteam, the ſame are ſhown in the following table:

Table of the relative preſſures per ſquare inch, temperatures and expanſibility of ſteam at degrees of heat above the boiling point of water, beginning with the temperature of ſteam of an elaſtic force equal to five pounds per ſquare inch, and extending to ſteam able to ſuſtain forty pounds on the ſquare inch.

Table of preſſures, temperatures and expanſions of ſteam equal in force to the atmoſphere.

	Pounds per ſquare Inch.		Degrees of Heat.		Expanſibility.	
Steam of an elaſtic force predominating over the preſſure of the atmoſphere upon a ſafety-valve,	5	requires to be maintained by a temperature equal to about	$227\frac{1}{2}$	and at theſe reſpective degrees of heat, ſteam can expand itſelf to about	5	times its volume, and continue equal in elaſticity to the preſſure of the atmoſphere.
	6		$230\frac{1}{4}$		6	
	7		$232\frac{3}{4}$		7	
	8		$235\frac{1}{4}$		8	
	9		$237\frac{1}{2}$		9	
	10		$239\frac{1}{2}$		10	
	15		$250\frac{1}{2}$		15	
	20		$259\frac{1}{2}$		20	
	25		267		25	
	30		273		30	
	35		278		35	
	40		282		40	

And ſo in like manner, by ſmall additions of temperature, an expanſive power may be given to ſteam to enable it to expand to 50, 60, 70, 80, 90, 100, 200, 300, or more times its volume, without any limitation but what is impoſed by the frangible nature of every material of which boilers and the other parts of ſteam-engines have been or can be made; and prudence dictates that the expanſive force ſhould never be carried to the utmoſt the materials can bear, but rather be kept conſiderably within that limit.

Upon this diſcovery, Mr. Woolf has obtained a patent for various improvements in the ſteam-engine, from the ſpecifica- tion

tion of which, the following extract is made, which will be sufficiently instructive to those who are acquainted with the subject.

Extract from Mr Woolf's specification.

"If the engine be constructed originally with the intention of adopting my said improvement, it ought to have two steam vessels of different dimensions, according to the temperature or the expansive force determined to be communicated to the steam made use of in working the engine; for the smaller steam vessel or cylinder must be a measure for the larger. For example, if steam of forty pounds the square inch is fixed on, then the smaller steam vessel should be at least one fortieth part the contents of the larger one; each steam vessel should be furnished with a piston, and the smaller cylinder should have a communication both at its top and bottom (top and bottom being here employed merely as relative terms, for the cylinders may be worked in a horizontal or any other required position, as well as vertical): the small cylinder, I say, should have a communication both at its top and bottom with the boiler which supplies the steam, which communications, by means of cocks or valves of any construction adapted to the use, are to be alternately opened and shut during the working of the engine. The top of the small cylinder should have a communication with the bottom of the larger cylinder, and the bottom of the smaller one with the top of the larger, with proper means to open and shut these alternately by cocks, valves, or any other well-known contrivance. And both the top and bottom of the larger cylinder or steam vessel, should, while the engine is at work, communicate alternately with a condensing vessel, into which a jet of water is admitted to hasten the condensation, or the condensing vessel may be cooled by any other means calculated to produce that effect. Things being thus arranged, when the engine is at work, steam of high temperature is admitted from the boiler to act by its elastic force on one side of the smaller piston, while the steam which had last moved it has a communication with the larger steam vessel or cylinder, where it follows the larger piston now moving towards that end of its cylinder which is open to the condensing vessel. Let both pistons end their stroke at one time, and let us now suppose them both at the top of their respective cylinders, ready to descend; then the steam of forty pounds the square inch entering above the smaller piston will carry it downwards,

wards, while the ſteam below it, inſtead of being allowed to eſcape into the atmoſphere or applied to any other purpoſe, will paſs into the larger cylinder above its piſton, which will take its downward ſtroke at the ſame time that the piſton of the ſmaller cylinder is doing the ſame thing; and while this goes on, the ſteam which laſt filled the larger cylinder, in the upward ſtroke of the engine, will be paſſing into the condenſer to be condenſed during the downward ſtroke. When the piſtons in the ſmaller and larger cylinder have thus been made to deſcend to the bottom of their reſpective cylinders, then the ſteam from this boiler is to be ſhut off from the top and admitted to the bottom of the ſmaller cylinder, and the communication between the bottom of the ſmaller and the top of the larger cylinder is alſo to be cut off, and the communication to be opened between the top of the ſmaller and the bottom of the larger cylinder; the ſteam, which in the downward ſtroke of the engine filled the larger cylinder, being now open to the condenſer, and the communication between the bottom of the larger cylinder and the condenſer ſhut off; and ſo on alternately, admitting the ſteam to the different ſides of the ſmaller piſton, while the ſteam laſt admitted into the ſmaller cylinder paſſes alternately to the different ſides of the larger piſton in the larger cylinder, the top and bottom of which are made to communicate alternately with the condenſer. Extract from Mr. Woolf's ſpecification.

"In an engine working with the improvements which have been juſt deſcribed, while the ſteam is admitted to one ſide of the piſton in the ſmaller cylinder, the ſteam on the other ſide has room made for its admiſſion into the larger cylinder, on one ſide of its piſton, by the condenſation going on on the other ſide of the large piſton which is open to the condenſer; and that waſte of ſteam which takes place in engines worked only by the expanſive force of ſteam, from ſteam paſſing the piſton, is prevented; for all ſteam that paſſes the piſton in the ſmaller cylinder is received into the larger.

"In ſuch an engine, where it may be more convenient for any particular purpoſe, the arrangement may be altered, and the top of the ſmaller made to communicate with the top of the larger, and the bottom of the ſmaller with the bottom of the larger cylinder; in which caſe the only difference will be, that when the piſton in the ſmaller cylinder deſcends, that

in the larger will afcend, and while the latter defcends the former will afcend, which for fome particular purpofes may be more convenient than the arrangement before defcribed."

Mr. Woolf then proceeds to defcribe various other modifications of his invention, and points out means for applying his improvements to the working of fteam-engines already conftructed and now in ufe. It is obvious that the advantages of this difcovery promife to be very confiderable, and I fhall take the earlieft opportunity of communicating the facts as they fhall be brought forward in practice.

VII.

Defcription of a Chemical Lamp, with double concentric Wicks. Communicated by Mr. Frederic Accum.

Defcription, &c. of an improved chemical lamp.

THOSE who are familiar with Chemiftry will readily allow, that one of the principal obftacles which frequently impede the progrefs of the young chemift in the profecution of his fcience, is the want of proper apparatus: he is at a lofs to felect from the number of inftruments difplayed in the lectures of his teacher, thofe which are calculated for rendering, *in his own apartment*, the moft important truths of the fcience legitimate by experiment.

It muft naturally be fo, as long as chemifts are anxious to exhibit a variety of coftly and complicated apparatus, and continue to pay that frivolous regard to *fhow* which characterifes fo many public lectures; and as long as the ftudent is told in the introductory leffon, that the fcience cannot be learned but in the laboratory, fitted up with furnaces, ftills, bellows, water-baths, fand-baths, *&c.* It is true indeed, that many chemical phenomena cannot be accurately obferved without the help of inftruments calculated to affift the imperfection of our fenfes; but it is equally true, that many of the brilliant apparatus which are daily difplayed in the laboratories of teaching chemifts, as inftruments of refearch, ferve more to divert the attention of the auditors, than to elucidate the fundamental truths of the fcience.

The modern proceffes of philofophical enquiry differ fo much from what they formerly were, and the inftruments of experi-

ment

ment have been so much improved during our own time, that by means of a comparatively small number of them, the most complicated processes may very commodiously and conveniently be carried on in the closet of every cultivator of chemistry.

Description, &c. of an improved chemical lamp.

To awaken the industry of the junior class of chemists, I gave in the twenty-fourth number of this Journal, a description and sketch of a convenient and portable universal furnace, and in the twenty-third number of the same Journal, I ventured to recommend to the young analyst a cheap and useful apparatus for drying the products of his analysis, and also convenient for experiments on artificial cold, &c. The favourable reception which these trifles have met with amongst those amateurs of the science for whom I have caused them to be constructed, has encouraged me to point out to them an improved lamp-furnace, by means of which, in the small way and on the table of the student, almost every operation may be performed at a cheap rate, as well as with facility and dispatch. In order to enable the intelligent reader to judge for himself, I shall first detail the construction of the instrument, and then point out to the inexperienced operator some of the most capital processes which serve to unfold the fundamental truths of the science, and for which the lamp may be applied according to the conditions laid down.

Fig. 1, *Plate* XIV. is a perspective view of the improved chemical lamp-furnace. It consists of a brass rod screwed to a foot of the same metal, loaded with lead. On this rod (which may be unscrewed in the middle, for rendering it more portable) slide three brass sockets with straight arms, terminating in brass rings of different diameters. The largest measures four inches and a half. These rings serve for supporting glass alembics, retorts, Florence-flasks, evaporating-basons, gas-bottles, &c. for performing distillations, digestions, solutions, evaporations, saline fusions, concentrations, analyses with the pneumatic apparatus, &c. If the vessels are not wished to be exposed to the naked fire, a copper sand-bath may be interposed, which is to be previously placed in the ring. Each of the brass-rings may, by means of a thumb-screw acting on the rod of the lamp, be set at different heights, or turned aside, according to the pleasure of the operator. Below these rings is a fountain-lamp on Argand's plan, having a metallic valve within,

Description, &c. of an improved chemical lamp.

within, to prevent the oil from running out while the reservoir is put into its place. This lamp also slides on the main brass rod by means of a socket and thumb-screw. It is therefore easy to bring it nearer, or to move it farther, at pleasure, from the vessel which may remain fixed; a circumstance which, independent of the elevation and the depression of the wicks of the lamp, affords the advantage of heating the vessels by degrees after they are duly placed, as well as of augmenting or diminishing the heat instantly; or for maintaining it for several hours at a certain degree, without in the least disturbing the apparatus suspended over it. It may therefore be used for producing the very gentle heat necessary for the rectification of ethers, or the strong heat requisite for distilling mercury.

The chief improvement of this lamp consists in its power of affording an intense heat by the addition of a second cylinder added to that of the common lamp of Argand. This additional cylinder incloses a wick of one inch and a half in diameter, and it is by this ingenious contrivance, which was first suggested to me by Mr. Webster, that a double flame is caused, and more than three times the heat of an Argand's lamp of the largest size is produced. This part of the construction of the lamp is clearly shewn in *Fig.* 2, which represents the concentric wick-holders of the lamp; the distance between the exterior and interior cotton is half an inch, the circumference of the largest wick is 4¼ inches, and that of the smaller two. Both the wick-frames are connected by a fine screw cut upon a piece of pinion-wire.

Fig. 3. is a section of the concentric cylindrical tubes in which the wicks move.

The superior advantages of this lamp, above all others I am acquainted with, consists therefore in quickly producing, if required, a very low as well as intense heat, and in regulating its power instantaneously; by means of which the operator may observe a number of minute circumstances essential to be known, but which cannot be noticed when the same process is carried on within a furnace.

Use of the Lamp-Furnace.

From what has been stated it is obvious, that this lamp may be used for a variety of chemical operations, if conducted under the conditions here pointed out; a few of which are—

1. For

1. For obtaining oxigen gas, by means of the glafs retort, from a mixture of four parts of finely powdered black oxide of manganefe, and three of concentrated fulphuric acid; or by merely heating in a retort, hyper-oxigenized muriate of potafh. Defcription, &c. of an improved chemical lamp.

2. For difengaging nitrogen, or azote, from animal fubftances, by affufing two parts of nitric acid of commerce, diluted with an equal bulk of water, upon one part of mufcular flefh (a piece of beef or veal cut into fmall pieces), and heating the mixture to ebullition in a glafs retort.

3. For obtaining hidrogen, fulphurated and phofphorated hidrogen, in the ufual manner; or for producing heavy carbonated hidrogen by ftrongly heating, in a retort, a mixture of three parts, by weight, of concentrated fulphuric acid, and one of fulphuric ether, or two of alcohol; nitrous gas, by any of the ufual proceffes; gazeous oxide of nitrogen, by decompofing nitrate of ammonia by heat in a retort; fulphureous acid gas, by caufing four parts of concentrated fulphuric acid to act on one of lump-fugar, affifting the action of the mixture in the retort by heat; muriatic acid gas; oxigenized muriatic acid gas, and carbonic acid gas, according to any of the ufual methods; ammoniacal gas, by heating in a retort a mixture of two parts of finely powdered lime and one of muriate of ammonia.

4. For the diftillation of nitric, muriatic, oxi-muriatic, acetic, oxalic, arfenic, pruffic, fuberic, mucous, and camphoric acids, according to the methods recommended by fyftematic writers.

5. For the production of metallic, earthy, and alkaline fulphurets; fuch as fulphuret of potafh, foda, barytes, ftrontia, ammonia, iron, copper, tin, mercury, &c.

6. For performing the analyfis of ores of gold, filver, copper, lead, zinc, tin, &c. and for examining mineral and native falts, earth and ftones, according to the methods pointed out in the "Practical Effay on the Analyfis of Minerals," and for a variety of other operations, too numerous to be detailed.

Old Compton-Street, Soho,
July 18, 1804.

On

VIII.

On the mutual Precipitations of Metallic Oxides. By J. L. GAY-LUSSAC.*

The subject is hitherto but little known.

THAT we have still so little information on the mutual precipitations of metallic oxides, can only be attributed to the complication of the results which they offer. Indeed, a little reflection will convince us that the oxidation, the affinity, the reciprocal action of the oxides, and the property which they have of neutralizing the acids unequally, are so many causes which must join in the production of the phenomena. It would, nevertheless, be very useful to know the order in which the metallic oxides precipitate from their solutions: the chemical analyses, and more especially the purification of metallic salts, would thence become easier. It was with this intention that I have made some experiments; and if they have not been sufficiently numerous to have enabled me to distinguish the influence of every cause, they, at least, show that of some, and will serve to draw the attention to a subject which is still obscure and very complicated.

Utility of the investigation.

I shall begin by relating the results which I have obtained, and shall afterwards endeavour to determine their causes.

Experiments with the green and red muriates of iron.

Having taken a solution of green muriate of iron, I added to it a little red muriate of the same metal, and I poured potash into the mixture in a quantity at least sufficient to decompose all the red muriate separately. After agitation, the first portions of alkali yielded a precipitate of iron very much oxided, without any mixture of black oxide; but by adding more and more of the alkali, it finished by being composed of the two oxides. The filtered liquor was then perfectly limpid, and no longer produced a blue with the prussiates, nor a black with the gallic acid: this proves that the very oxided iron had been precipitated from it. On making the inverse experiment, that is to say, on putting a little green muriate of iron into a larger quantity of red muriate, and precipitating it by the alkali, the black oxide was retained in the solution to the last, and was not precipitated until after all the red oxide. Hence therefore it results that the black oxide

* From the Annales de Chimie, No. 145. or Vol. XLIX. p. 21.

of

of iron precipitates the red oxide, and that it is, consequently, very easy to have green solutions of iron without red oxide. Black oxide of iron precipitates the red.

Into a solution of sulphate of zinc of commerce, which is known to contain much iron, I poured a little potash, to produce a precipitate, and I agitated and heated the mixture. On examining the precipitate I found oxide of zinc and a little iron very much oxided, and nevertheless the liquor still contained much iron, but it was at the minimum of oxidation: an addition of alkali only separated oxide of zinc. I divided the filtered liquor into two portions; into one I poured oxigenated muriatic acid, and I boiled the other with a little nitric acid. Potash then poured into the two liquors separated all the iron from them, so that there only remained a very pure sulphate of zinc, containing only a little sulphate of potash, which it is easy to avoid by employing oxide of zinc, recently prepared and well washed, to separate the iron. On making the same experiments on other solutions of zinc, I constantly found that oxide of zinc precipitated red oxide of iron, and that, on the contrary, it was precipitated by the black oxide. Experiments with the sulphate of zinc of commerce. Oxide of zinc precipitates red oxide of iron, but is precipitated by the black.

A solution of zinc in nitric acid may be directly obtained, sufficiently pure, by dissolving it very rapidly: great part of the very oxided oxide of iron is precipitated; and that which remains in solution requiring a great excess of acid, is precipitated by dissolving another quantity of zinc. But if the solution has been made slowly, it retains much iron, which being but little oxided, is retained very strongly.

It is well known that, in all experiments of this kind, the precipitate may be composed of one or of two oxides, according to the quantity of alkali employed; but the better to observe what passes, it is adviseable to put very little alkali into the metallic solution, the precipitate being in that case composed of only one oxide. The quantity of alkali employed should be small.

By continuing to follow the same processes I found, that when the iron is very much oxided it is precipitated by oxide of copper, and that the inverse takes place when it is but little so. Here are two very important consequences, because they may be very frequently applicable in the arts: the first is, that all the iron may be separated from a solution of copper; the second, that all the copper contained in a green solution of iron may be abstracted. Importance of the mutual precipitations of copper and iron.

Several

Sulphate of copper may be freed from iron:

Several colours are prepared with sulphate of copper, but the iron which it always contains, and which hitherto has never been completely separated, alters the shade. If, to accomplish this last object, the iron is strongly oxided by means of nitric acid, or, which is better, of oxigenated muriatic acid, the sulphate of iron may be intirely precipitated by pouring a sufficient quantity of potash into it, and by heating and agitating the liquor.*

[illegible] of [illegible] copper.

Green sulphate of iron is also frequently employed in the arts, and in many of them it is desirable that it should not retain any copper. Iron has the property of separating it, but it appears that it only does so imperfectly, and requires much time. It would doubtless be more advantageous to employ potash, and to pour a little into the green sulphate: the precipitate of black oxide of iron would be speedily re-dissolved by agitation, and would precipitate, at the same time, the oxide of copper and the red oxide of iron, if there was any in the green sulphate.

Ammonia dissolves oxide of iron at a minimum.

I shall here remark that, having employed ammonia to discover the copper in the green sulphate, I observed that, on adding an excess of alkali, the oxide of iron was dissolved in great abundance, though it is known that, in the same circumstances, it does not dissolve the very oxided iron. The solution left in the air is decomposed, the ammonia escapes, and a black crust is formed on the surface of the liquid which soon defends it from the contact of the air. In analyses, ammonia is frequently employed to separate iron, but this method is not good unless it had been strongly oxided before. This induces me to believe that this circumstance might have prevented Bergman from separating iron from nickel by means of ammonia; for he found that his solution contained the oxides of both metals, and this could only have arisen from the iron not having been sufficiently oxided.

Oxigenated muriate of mercury precipitates the red oxide of iron and those of zinc and copper.

I also discovered, by the same means, that the oxide of oxigenated muriate of mercury precipitates the red oxide of iron with the greatest facility, and those of zinc and copper from their muriatic solution.

Oxide of silver precipitates oxide of copper.

Having dissolved a piece of silver in nitric acid, I obtained a blue liquor composed of copper and silver. A little potash poured into the solution formed a flocculent precipitate, composed,

posed, in great part, of oxide of silver, because the precipitation took place only where the alkali was poured in; but this precipitate was gradually covered with oxide of copper, and, by agitation, it was in a little time replaced by the latter. An addition of alkali having given me a precipitate of oxide of silver which was not re-dissolved, I filtered it, and obtained a liquid perfectly colourless, which did not contain any more copper. If it is wished to avoid the potash in the solution, a part of the impure nitrate of silver may be decomposed separately, and the precipitate, well washed, may be employed to separate the copper of the other part. This simple method of separating copper from a solution of silver may be very useful in laboratories, and even in large works.

The oxide of silver also decomposes the nitrate of zinc; and the oxide of manganese the muriate of copper.

The precipitates are not pure oxides.

In what precedes I have, for shortness, supposed that the precipitates were pure oxides; but I am far from believing this to be the case; on the contrary, I consider nearly all of them as true salts. Copper, for example, was always precipitated of a bluish-green, although the shade varied with the oxides which were precipitated; and it is now well ascertained by Proust and the younger Berthollet, that the green and blue oxides of copper retain some of the acid.

Such are the facts as I have observed them, and on which alone I shall make some reflections. Although they are too few to have enabled me to fix on all the circumstances which concurred in their production, the examination of them will, however, develope some of them.

Inferences from a consideration of the state of the acidity.

In fact, if we direct our attention to the acidity of the different salts noticed above, we shall see,

Neutralization of the acids by oxides.

1*st*. That the iron which is but little oxided, and the highly oxided mercury which precipitates the red oxide of iron, the oxide of zinc, and that of copper, approach nearer to neutralization than the latter.

2*d*. That the zinc and manganese which precipitate the copper, neutralize the acids better than it *.

* By neutralizing the acids more or less, I mean that property possessed by the metallic oxides, and some earths, such as glucine and alumine, of approaching more or less, in their combinations with the acids, to the term of neutralization.

3*d.* That the oxide of filver which precipitates thofe of zinc and copper, neutralizes the acids better than them.

Alumine is precipitated by metallic oxides;

If, befides, we reflect, that alumine, whofe folutions are very acid, is precipitated by feveral metallic oxides which neutralize the acids better than it; that, according to the experiments of Vauquelin, glucine decompofes aluminous falts,

and by glucine.

and that its folutions are more neutral than thofe of alumine, although they are not entirely fo; and, finally, that magnefia,

Magnefia precipitates the earths and oxides.

which neutralizes the acids perfectly, precipitates the preceding earths, and a very great number, not to fay all, the oxides from their folutions; we cannot abftain from allowing that, if the property poffeffed by the metallic oxides and feveral earths of neutralizing the acids unequally, is not the only caufe of the decompofitions which I have detailed, it is at leaft one of the principal.

The affinity for oxigen is not the caufe.

We may alfo conclude from the fame experiments, that the metals which have a great or a weak affinity for oxigen, do not enjoy any particular property with refpect to their mutual precipitations; for we fee that iron, in a ftate of great oxidation, is precipitated by a number of oxides which it precipitates when it is lefs fo; and, that there are feveral oxides which contain lefs oxigen than that of zinc, which precipitate the latter, while there are others which are precipitated by it.

The affinity of the different metals for oxigen is therefore rejected as the caufe of the mutual precipitations of their oxides; but can the greater or lefs oxidation of the fame metal occafion a variation in the affinity of the oxide for the acids? This opinion has been promulgated by Cit. Berthollet in his Chemical Statics *, and he has grounded it upon feveral facts,

The ftate of the oxidation influences the affinity of the oxides for the acids;

in which the metal, by lofing a little of its oxigen by any means whatever, forms another falt with lefs acid. This happens to the oxigenated muriate of mercury, which, by expofure to light, or by being brought into contact with iron, is changed into white muriate by abandoning fome of its acid. Although thefe, and other fimilar facts, are capable of a different interpretation, other confiderations, which I fhall omit here, becaufe they would lead me too far, induce me to participate in the opinion of Citizen Berthollet; but I do not believe

* A tranflation of which will be publifhed by Mawman, in the Poultry, about the middle of the prefent month.

lieve that this cause can have much effect, it being strongly counteracted by the acidity which almost all the metallic solutions possess, and by the insolubility of the oxides. Thus, although it appears to me that iron, when little oxided, has more affinity for muriatic acid than when it is greatly oxided, I should rather attribute the precipitation of the latter by the former to the great excess of acid which its solution requires, than to its weaker affinity.

Neither, for the same reasons, do I believe that the affinity of the different oxides for the acids, an affinity which I measure, with Cit. Berthollet, by the capacities for saturation, can be considered as the cause of their mutual precipitations. but is not the cause of their mutual precipitations.

Besides, there is one consideration of some importance which should be taken into the explanation of the mutual precipitations of the oxides; it is that, in a case where the precipitation of a metallic solution is produced by means of an alkali, the precipitate retains some of the acid which can favour its solution; so that an oxide which could retain much of the acid, would dissolve more readily than that which could only retain less. It must really be so with iron, which, when it is precipitated from a green solution, retains much more acid than when it is precipitated from a red solution, and which dissolves much better in the acids in the first case than in the second. This more ready solution cannot, however, be considered as a cause of the mutual precipitations of the metallic oxides; it may be very favourable to them, but cannot determine them. In fact, we see that the oxide of copper, which retains much acid, is nevertheless precipitated by oxide of silver, which does not sensibly retain any, when they are precipitated from their nitric solutions by potash. Influence of the retained acid.

CONCLUSION.

THE metallic oxides are mutually precipitated from their solutions. Several causes may contribute to this; but among the number of the principal must be placed the property which they have of neutralizing the acids unequally. General inferences.

This property has furnished us with the means, *1st.* Of freeing a green solution of iron from the red oxide which it may contain; *2d.* Of separating the sulphate of zinc and that of copper from the iron which is always found in them; *3d.* Of having a green sulphate of iron free from copper; *4th.* Of

 readily

General inferences.

readily feparating the copper from a folution of filver. It is eafy to accumulate thefe applications by extending them to a greater number of fubftances. Thus, the oxides of cobalt and nickel do not neutralize the acids equally; that of the two which neutralizes it moft, will be able to precipitate the other, and remain alone in the folution. Thus, alfo, fince glucine neutralizes the acids much better than highly oxided iron, it will be eafy to feparate this metal from its folutions, by firft oxidating it ftrongly, and afterwards precipitating one part of the folution, to employ it, after being well wafhed, to precipitate the iron of the other part.

The greater or lefs affinity of the metals for oxigen does not give them any particular property with refpect to the mutual precipitation of their oxides.

Oxidation produces a variation in the affinity, or the capacity for faturation of the oxides for the acids; neverthelefs, the refults are only fenfible inafmuch as they produce a change in the neutralization, and in this cafe they may be attributed to the latter caufe.

The affinity of the oxides for the acids may indeed contribute to their mutual precipitations, but its effects are very limited.

It appears, therefore, in general, that, all circumftances remaining otherwife the fame, the fubftances which neutralize the acids beft, may precipitate the others from their folutions.

I repeat in concluding, that it is only on the facts which I have related that I have eftablifhed my reafoning, and that it was not my object in this note to fpeak of the precipitations by the metals, nor of thofe which are owing to the reciprocal action of the oxides, or to that of the latter and the alkalis.

A Report

IX.

A Report of the State of his Majesty's Flock of Fine Wooled Spanish Sheep, for the year ending Michaelmas, 1803. *By the Right Honourable Sir* JOSEPH BANKS, *Bart. P. R. S.* &c. &c. *From the printed Copy communicated by the Author.**

THE wether lambs of the last year having been sold in their wool, and the rams wool retained, in order that two years growth might be prepared for sale together, his Majesty's Spanish flock consisted, when shorn in June 1802, of ninety-six ewes only; the fleeces of these, after having been washed on the sheeps' backs as usual, weighed as follows: State of his Majesty's flock of Spanish sheep, June 1802.

In wool, as shorn from the sheep - -	352 lbs.
Loss in scowering - - - - -	96
Amount of scowered wool - - -	256

This wool, when sorted, produced as follows:

Prime wool, or R.	221 lbs. at 5s. 9d.	£.63	10	9
Choice locks, or F.	32 —— 3 6	- 5	12	0
Fribs, or T. -	3 —— 1 9	- 0	5	3
		£.69	8	0

After deducting the expence of sorting and scowering, at the high rate which an individual who is not a manufacturer must pay for these processes, this wool is worth about 5*l.* a tod, or 43*l.* 5*s.* a pack, as clipped from the sheeps' back.

The prime wool was purchased by John Maitland, Esq. member of parliament for Chippenham, whose mercantile house, established for more than a century, has always dealt largely in the importation of Spanish wool, and who from the first introduction of Merino sheep into this country by the King in the year of 1787, has uniformly given the most liberal and zealous aid to the promotion of his Majesty's patriotic views, though doubtful in the beginning of the ultimate success of the project. Sale of the wool;

* For the original project of this important undertaking and its subsequent progress, see the former reports by the same Right Hon. Gentleman, inserted in our Journal, quarto, vol. IV. p. 289, and octavo, vol. V. p. 65.

It

manufactured into cloth; It was made into cloth by Mr. Edridge, a manufacturer of Chippenham, whose skill and respectability in his line is exceeded by no man. He inspected its quality with the most minute exactness, and with an eye more inclined to expect symptoms of degeneration than of improvement, during the whole of the numerous processes to which wool is subjected in the making of broad cloth, and he found that in every one of them it answered to his complete satisfaction.

The cloth made from this wool proved so excellent in its kind, that the King was graciously pleased, at the desire of Mr. Maitland and Mr. Edridge, to permit these gentlemen to explain, in his Majesty's presence, its qualities and peculiarities.

of which samples may be seen. Samples of this cloth may now be seen in Mr. Maitland's warehouse in Basinghall-street; and it will be found, in conversing with Mr. Maitland and his partners, that in their opinion the R.'s of his Majesty's wool, considered as a pile, are inferior to but few of the best of those imported from Spain, though it is probable that no pile in Spain throws out so small a proportion of F.s and T.s. From this opinion it may fairly be deduced, that his Majesty's wool has improved since the sheep were imported from Spain; indeed there is every reason to believe that it is still improving, and will in a very few years equal if not excel the very best piles that have hitherto been imported into this kingdom.

The wool has improved.

Improvement of the carcase of Merino sheep, by Mr. Tollet, without damaging the wool. Mr. Tollet, a gentleman of Gloucestershire, who has purchased Merino sheep both from the King and from Lord Somerville, has been very successful in improving the carcase without damaging the wool; he possesses a ram, bred from a ram and a ewe both purchased from the royal flock in 1801; which, when clipped in June last, yielded 11 lbs. 12 oz. of unwashed wool. The carcase of this sheep was then estimated by good judges at 16 lbs. a quarter, and it was admitted to be a handsome sheep.

For this animal Mr. Tollet has refused an offer of 200 guineas, or of 100 for the next season's use of him; he also refused 30 guineas each for the sire and the dam, though old and infirm, being unwilling to part with animals which had belonged to the royal flock; he however sold their ram lamb of the last year for 30 guineas, and thus made some progress in ascertaining the value of this important breed.

These facts, which prove an amelioration in the King's Merino sheep, are fully confirmed by the improved shape and weight of his Majesty's sheerling rams of the present year, and give a justifiable hope, that by a due selection of rams and ewes, and a correct judgment in matching them, Merino sheep will in time be produced, with carcases perfectly fashionable, and wool as perfectly fine. Confirmation by the improvement of his Majesty's flock.

No purchaser having been last year found for the lambs wool at a price adequate to its value, it was made into light ladies cloth, which proves excellent, and promises to be a valuable article. A speculation, however, has offered for manufacturing the lambs wool into superfine woolen hose, which seems likely to yield a still better price for the raw article than the cloth. Lamb's wool.

The demand for his Majesty's Merino sheep increases at present beyond all calculation. The best informed clothiers in Gloucestershire, enlightened no doubt by the useful labours of the Bath Society, and the valuable experiment of Dr. Parry, as well as by the Doctor's, and by Lord Somerville's publications, are among the most anxious applicants to purchase. The Bath Agricultural Society, whose attention has been most particularly directed to the improvement of English wool, humbly requested the King to give them a Spanish ram, which request his Majesty most graciously complied with last Autumn, and they returned thanks in the warmest terms of respectful gratitude and satisfaction. The demand from the growers proves that these facts are well established;

As speculation on the value of Spanish sheep is evidently on the increase, and a reasonable probability now appears that his Majesty's patriotic exertions in introducing the breed, will at last be duly appreciated and properly understood, it would be palpably unjust should the views of those who wish to derive a fair advantage from the sale of the progeny of Spanish sheep purchased by them from the royal flock, be in future impeded by a continuation of the sale of the King's sheep at prices below their real value. and render it just that the sheep should be distributed by the competition of a public sale.

This circumstance having been stated to the King, his Majesty was graciously pleased to permit the rams and ewes that are to be parted with from the royal Merino flock this year, to be sold by auction, in the same manner as is done at Woburn by his grace the Duke of Bedford, and at Holkham by Mr. Coke, on the presumption of this being the most likely manner of

placing

placing the beſt individuals of their improved breeds in the hands of perſons moſt likely to preſerve, and further to improve them.

JOSEPH BANKS.

Auguſt 17, 1803.

POSTSCRIPT.

Poſtſcript.

AS the publication of this report has been delayed by unavoidable circumſtances to ſo late a period, it is proper to add, that the wools of 1803 have yielded, both raw and ſcowered, much as uſual. The prime or R. of the ewe flock were ſold for 6*s*. 9*d*. a pound, and that of the Rams for 6*s*. 6*d*. Theſe enormous prices, however, depended on a ſcarcity of imported Spaniſh wool, and are highly diſtreſſing to the manufacturer: they ought not, therefore, to be allowed to enter into the ſpeculation of the grower.

Notice of ſale of the ſheep that can be ſpared.

The ſheep that can be ſpared from the royal flock will be ſold by auction this year at a barn oppoſite the Pagoda in Kew-lane, on the 15th of Auguſt next. Notice of the particulars will be given as ſoon as poſſible.

July 10, 1804.

X.

On the Oxides of Lead. By THOMAS THOMSON, *M. D. Communicated by the Author.*

Great importance of the analyſis of metallic oxides:

PERHAPS there is no practical branch of chemical inveſtigation of more importance than the analyſis of the metallic oxides. Almoſt every thing relating to the metalline paints and ſalts depends upon it; and it involves, either directly or indirectly, moſt of the intereſting queſtions in the theoretical department of chemiſtry.

firſt applied with effect by Bergman and Scheele.

Bergman and Scheele were, I believe, the firſt perſons who applied chemical analyſis, in the modern ſenſe of the phraſe, to the metallic oxides. Notwithſtanding the difficulties with which theſe illuſtrious chemiſts had to ſtruggle, their experiments were made with ſo much care and ſagacity, that they ſtill furniſh us with the beſt data for aſcertaining the compoſition of ſeveral of theſe bodies. No modern chemiſt has laboured

boured so successfully in this department as Mr. Proust. To him we are indebted for the analysis of the oxides of zinc, iron, tin, copper, antimony, and arsenic. His dissertations are all stamped with the character of originality, and display so much skill and candour, that they never fail to command the confidence of the reader. If he sometimes pushes his consequences a little too far, he more than compensates for this by the originality of his views, and the new light which he throws upon every subject that he discusses. Analyses of Proust.

I intend at present to offer some observations on the oxides of lead, a subject more than once slightly touched upon by Proust, but never fully discussed by him. I trust the difficulty of the subject will plead my excuse, if I shall be unfortunate enough to fall into mistakes. Subject of this memoir: the oxides of lead.

We are acquainted with three oxides of *lead* sufficiently distinct from each other. The first is of a yellow colour, and forms the base of almost all the salts of lead; the second is a paint well known by the name of *red lead;* the third a brown powder discovered by Scheele, and examined more lately by Proust and Vauquelin. Besides these three, a fourth has been announced by Proust; and *litharge* has been considered by some as constituting a fifth. Let us examine these oxides. Three distinct oxides; yellow, red, brown; and two others.

I. *Yellow Oxide.*

The yellow pigment called *massicot* consists essentially of this oxide; but the easiest method of forming it, is to dissolve lead in nitric acid. Pure lead dissolves completely in that acid; but the lead of commerce usually leaves a small quantity of grey powder, which consists for the most part of oxide of antimony, sometimes mixed with a little silica. When the solution is concentrated by evaporation, we obtain crystals of nitrate of lead, a salt too well known to require any particular description. Yellow oxide of lead.

1. When the crystals of nitrate of lead thus obtained by evaporation, and well dried upon blotting-paper, are exposed to a temperature of about 300°, they lose, at an average, three *per cent.* of their weight. This loss is not to be ascribed to the escape of mere water, for the fumes smell strongly of nitric acid. Nitrate of lead, loses acid by low heat:

2. When 69 grains of lead are dissolved in nitric acid, and the solution evaporated to dryness, the nitrate of lead, after — lead contained therein.

being

being dried at the temperature of 300°, weighs 112 grains. Hence 100 grains of lead yield $162\frac{1}{3}$ grains of nitrate of lead. From this we learn that 100 parts of nitrate of lead consist of

$61\frac{2}{3}$ lead,
$38\frac{1}{3}$ foreign bodies.

100.

Carbonate of lead: white lead.

3. When 112 grains of nitrate of lead (dried at 300°) are dissolved in water and mixed with a solution of carbonate of potash, a copious white powder precipitates, which is a *carbonate of lead*. Bergman shewed long ago, that the white lead of commerce is precisely the same with this carbonate. When washed, collected on a filter, and dried at 300°, it weighs 90 grains. This shews us that 69 grains of lead yield 90 grains of carbonate: of course, 100 grains of lead would yield $130\frac{1}{2}$ grains of carbonate. From this experiment we learn, that 100 parts of precipitated carbonate of lead are composed of

$76\frac{2}{3}$ lead,
$23\frac{1}{3}$ foreign bodies.

100.

Carbonate of lead by heat leaves yellow oxide; 9 lead + 1 oxigen.

4. When 90 grains of precipitated carbonate of lead are exposed in a retort to a heat gradually raised to redness, the acid and water which they contain are driven off, and a yellow coloured oxide remains behind. This oxide weighs 77 grains, and contains, of course, 69 grains of lead. Hence it follows, that the yellow oxide of lead is composed of 69 lead + 8 oxigen, or *per cent.* of

89.7 lead,
10.3 oxigen.

100.

— very fusible.

It is well known that the oxides of lead very easily melt and run into glass. This happens in the preceding experiment, unless particular care be taken. In that case the lead acts with great energy upon the retort; but the loss of weight is the same, unless the heat has been a great deal too high. When the oxide is fused in an earthen vessel, it covers the surface with a yellow glass, as in the coarsest kinds of pottery. In that case some of the oxide may be dissipated, unless the pro-

per

per precautions are taken. It deserves attention, that when carbonate of lead is slowly heated in a glass retort till it begins to melt, the melted portion has a fine yellow colour, while the colour of what remains in the state of a powder is a dirty pale brick-red; whereas in a platinum crucible the melted portion is red and the unmelted yellow. Singular fact.

5. From the preceding experiments it follows, that the yellow oxide of lead contains 10.3 *per cent.* of oxigen. Mr. Proust has deduced nine *per cent.* as the proportion of oxigen, from his experiments. This result does not differ much from mine. If I have committed an error, the oxigen I think is rated too high; for the lead which I used contained $1\frac{1}{2}$ *per cent.* of antimony, the oxides of which have much more oxigen than the yellow oxide of lead. Perhaps we shall come nearer the truth by taking the mean of the two results: we may therefore consider the yellow oxide of lead as composed of Corrected elements of the carbonate.

$90\frac{1}{2}$ lead,
$9\frac{1}{2}$ oxigen.

100.

6. The preceding experiments enable us to state the constituents of nitrate of lead as follows: Comp. parts nitrate of lead, &c.

1. *Nitrate dried on Blotting-Paper.*	2. *Nitrate dried at 300°.*
66 yellow oxide,	68.5 yellow oxide,
34 acid and water.	31.5 acid and water.
100	100.0

They give us also the precipitated carbonate of lead, dried at 300°, as follows:

86 yellow oxide,
14 acid and water.

100.

The native carbonate of lead contains about 16 *per cent.* of carbonic acid. Precipitated carbonate then either contains less acid than native, or it loses a part at a low heat. It is well known that carbonates, when in crystals, frequently contain more acid than when in the state of powders.

7. Yellow oxide of lead is a powder of a lively yellow colour, tasteless, insoluble in water, but soluble in fixed alkalies and Characters and habitudes of the yellow oxide.

acids,

acids. The alkaline solutions have a yellow tinge; but the acids are most frequently colourless. It readily melts when heated, and forms a yellow, semitransparent, brittle, hard glass. It does not lose oxigen gas when heated. In violent heats a portion of the oxide is dissipated. When kept heated in the open air, its surface becomes brick-red. When mixed with metallic lead it runs, according to Proust, into a green glaze.

Yellow oxide by mere heat from the nitrate.

8. The yellow oxide may be obtained directly from the nitrate of lead, by exposing that salt to a sufficient heat; but the loss of weight sustained is usually greater than it ought to be. I suspect that this is one reason why Proust found so small a proportion of oxigen in yellow oxide. One hundred grains of nitrate of lead (obtained by evaporation) were put into a small Wedgewood crucible furnished with a lid, and enclosed in a common earthen-ware crucible. They were exposed for half an hour to an intense red heat in a wind-furnace. The salt was converted into a very hard, yellow, brittle glass, nearly opake: It had sustained a loss of 40 *per cent.* or about six *per cent.* more than it ought to have lost. On breaking this glass to pieces the reason of this became obvious: It contained a great number of globules of lead reduced to the metallic state, some of them of considerable size. From this experiment we learn, that lead is reducible directly from the nitrate merely by the application of heat, without adding any combustible matter.

II. *Supposed first Oxide.*

Ashes of lead, or supposed first oxide. It is yellow oxide and metallic lead.

When lead is kept melted in the open air, it is soon covered with a dirty coloured powder, formerly called the *ashes* of lead. When this powder is heated sufficiently, it melts into a greenish yellow glass, in which globules of lead may be detected. Mr. Proust has shewn, that these ashes are a mixture of the yellow oxide of lead with lead in the metallic state. They do not, therefore, constitute a peculiar oxide.

Neither is the *white oxide* of the French chemists entitled to a place among the oxides of lead; being in all cases nothing more than the yellow oxide combined with an acid, usually the carbonic.

Proust's oxide by boiling lead in the nitrate.

But Mr. Proust, in his observations on the *Connoissances Chimiques* of Fourcroy, has mentioned the method of forming an oxide of lead containing less oxigen than the yellow. When lead

lead is boiled in a folution of nitrate of lead, the liquid gradually affumes a yellow colour, and, on cooling, depofits cryftals in fcales. Thefe cryftals, according to Prouft, contain the oxide in queftion: But his conclufions, as far as appears, were formed from the fingle experiment related. He does not feem to have decompofed the falt, nor to have examined its bafe.

1. When 100 grains of nitrate of lead are diffolved in water, and boiled in a phial with a cylinder of lead (weighing $64\frac{1}{2}$ grains), the metal foon lofes its brilliancy and is covered with a white cruft, while the liquid affumes a yellow colour. The boiling was continued (water being added as faft as it evaporated) till the liquid feemed to exert no farther action on the lead. The cylinder being then taken out and weighed, was found to have loft 44 grains. From this we learn, that 100 grains of nitrate of lead diffolved in water, are capable of uniting with 44 grains of lead, or almoft half their weight. The whole, however, was not diffolved. A bluifh-grey powder fell to the bottom, and increafed in quantity as the cylinder diminifhed. If thefe 44 grains were oxidized at the expence of the yellow oxide of the nitrate, we fhould have a new oxide containing much lefs oxigen; and it would be eafy to affign the proportion of its conftituents; for 100 grains of nitrate contain 66 grains of yellow oxide, compofed of $59\frac{3}{4}$ lead and $6\frac{1}{4}$ oxigen: Therefore the new oxide contains $59\frac{3}{4} + 44$ lead and $6\frac{1}{4}$ oxigen, or, *per cent.* The experiment.

94.3	lead,
5.7	oxigen.
100.0	

But it is extremely unlikely that the 44 grains of lead fhould receive the whole of the oxigen neceffary to enable them to diffolve from the oxide, while an excefs of nitric acid is prefent in the folution. Let us therefore examine the new falt.

2. When the folution cools, it depofits thin fcaly cryftals of a light yellow colour: They have the fame fweet aftringent tafte as common nitrate, but are lefs foluble in water. If the yellow liquid which remains be farther concentrated, it depofits, on cooling, fmall needles of a pale yellow colour, not unlike *fugar of lead*. Their tafte is fweet and aftringent; they are not altered by expofure to the air. When thrown into cold water they fall to the bottom, the liquid gradually be- Depofition of fcaly cryftals by cooling, and fmall needles by evap. White depof. of carbonate by cold water.

comes

comes milky, and depofits a white powder. This powder does not difappear, though the folution be heated boiling hot; but the liquid acquires the property of diffolving an additional quantity of the falt, without depofiting any more white powder. Boiling water diffolves the falt without any fimilar depofition. Hence I think we may conclude, that the white powder is owing to the prefence of fome carbonic acid in the cold water, and that our falt in this refpect refembles acetate of lead.

The falt depofited in the boiling, contained yellow oxide and an under proportion of acid;

3. When 30 grains of the falt depofited during the boiling of the lead in the nitrate, were cautioufly heated to rednefs, they melted into a yellow mafs, which weighed 24.5 grains. The lofs of 5.5 grains muft be afcribed to acid and water. Hence this falt is compofed of

81.5 oxide,
18.5 acid and water.

100.0

Thefe 24.5 grains of oxide being diffolved in nitric acid, yielded 35 grains of common nitrate of lead (dried at 300°). But 35 grains of nitrate contain 24 grains of yellow oxide, which fcarcely differs from the quantity diffolved. From this experiment it feems to follow, that the falt in queftion contains only yellow oxide, and that it differs from common nitrate in containing a fmaller proportion of acid. But it will be faid, perhaps, that the oxide of the falt abforbed oxigen from the nitric acid during the application of the heat, and was thereby oxidized up to the ftate of yellow oxide.

as alfo the cryftals do.

4. Twenty-three grains of the needle-form cryftals were diffolved in water and decompofed by carbonate of potafh. The carbonate had the common appearance, and, when dried in 300°, weighed 24 grains. But 24 grains of common carbonate contain about $21\frac{1}{2}$ of oxide, and ought therefore, when diffolved in nitric acid, to yield about $31\frac{1}{2}$ grains of common nitrate of lead (dried at 300°); and, upon trial, I found this to be the cafe very nearly. The oxide in the needle-form cryftals then is the yellow; for there is no apparent courfe from which, in the above experiment, oxigen could be drawn. And if this be the cafe with the needles, it muft be fo alfo with the fcaly cryftals; for the two falts are obvioufly the fame.

5. Sixty-

5. Sixty-three grains of the salt, partly in scales and partly in needles, were fused with carbonate of potash in a Wedgewood crucible. By solution and filtration a flesh-coloured powder was obtained, which was a mixture of oxide of lead and silica. It weighed 53 grains; but a portion which I could not estimate adhered to the crucible. The silica was obviously abraded from that vessel. Thirty grains of this powder digested in nitric acid, left $3\frac{1}{2}$ grains of silica; of course, $26\frac{1}{2}$ were dissolved. The solution yielded $39\frac{1}{2}$ grains of nitrate of lead. Now $39\frac{1}{2}$ of nitrate contain 27 grains of yellow oxide, or almost the very quantity dissolved. The oxide obtained by this experiment, then, was the yellow; of course, it coincides exactly with the preceding ores.

The oxide of Proust's salt is not different from the yellow.

Proust's salt, then, does not appear to contain a different oxide from common nitrate; but its new properties were owing to the different proportion of its acid. It is completely neutralized, whereas common nitrate contains an excess of acid, and is, in fact, a super-nitrate. But if this conclusion be well founded, Proust's nitrate may be formed by exposing common nitrate to a heat sufficient to expel the excess of acid. It was requisite to verify this presumption by experiment.

6. One hundred grains of nitrate of lead (dried in 300°) were exposed to a graduated heat in a flask. Fumes of nitrous acid separated in abundance, and the salt lost five *per cent.* of its weight. On increasing the temperature the salt melted into a transparent glass of a very pale yellow colour. The weight of the mass was now reduced to 85 grains. Hence it was composed of 68.5 oxide and 16.5 acid, or, *per cent.* of

Proust's salt made by abstracting acid instead of adding lead.

80	oxide,
20	acid.
100.	

On pouring water into the flask and digesting, I obtained a yellow solution similar to that formed by boiling lead in nitrate of lead, but not so deep. A yellow powder refused to dissolve; it consisted chiefly of the portion of salt at the bottom of the flask, which had been exposed to a higher temperature. It was tasteless, and not unlike sub-muriate of lead. When heated to redness it melted into a yellow glass, and lost 14 *per cent.* It was therefore composed of

86 oxide,
14 acid and water.

100.

The solution being evaporated, deposited two sets of crystals; one set consisting of common nitrate of lead, another set resembling those obtained by Proust.

Three nitrates of lead; *viz.* neutral, and with excess or with defect of acid.

7. From the preceding details we learn, that there are three distinct species of nitrated lead: The first is a *super-nitrate*, or contains an excess of acid; the second is neutral; the third contains an excess of base, and is, of course, a sub-nitrate. The first species includes the common nitrate of chemists in all its varieties; the second, the nitrate of Proust; the third, the yellow powder obtained by heating common nitrate sufficiently.

III. *Broun Oxide.*

Brown oxide; left when minium is dissolved in nitric acid, &c.

Though this oxide contains a maximum of oxigen, I beg leave to introduce it here, because the knowledge of its composition is necessary to enable us to analyse the *red oxide* of lead. It was discovered by Scheele, and described by him in his dissertation on manganese. When diluted nitric acid is poured upon red lead, the greater part of the oxide is dissolved, but a brown powder remains behind, which is not acted upon by the acid. This brown powder is the brown oxide of lead. Proust discovered that it may be formed also by causing a current of oxi-muriatic acid gas to pass through red lead suspended in water.

Its habitudes;

1. This oxide is a tasteless powder, of a flea-brown colour, and very fine and light. It is not acted on by sulphuric nor nitric acid. To muriatic it gives out oxigen, and converts it into oxi-muriatic acid. Oxi-muriatic acid dissolves it, and forms two salts, muriate and hyper-oxi-muriate of lead. The vegetable acids reduce it to the state of yellow oxide. Fourcroy, on the authority of Vauquelin, affirms, that sulphur takes fire when triturated with brown oxide of lead. With me the experiment did not succeed: I suspect, therefore, that the oxide used by Vauquelin contained a portion of hyper-oxi-muriate of lead mixed with it.

contains one-tenth more oxigen than yellow

2. When 100 grains of this oxide, prepared from red lead by nitric acid, are exposed to a red heat, they lose nine grains of

of their weight, and are converted into yellow oxide: These nine grains are oxigen gas. Hence brown oxide is composed of 91 yellow oxide and 9 oxigen. But 91 of yellow oxide contain 9.4 of oxigen.* Therefore 100 parts of brown oxide are composed of oxide; easily disengaged.

81.6 lead,
18.4 oxigen.

100.

3. Mr. Proust, from his experiments, states the proportion of oxigen in this oxide at 21 *per cent.* If we take the mean of the two results, we obtain 19.7. We may, therefore, lay down 20 *per cent.* as the proportion of oxigen in brown oxide of lead: This cannot deviate far from the truth.

IV. *Red Oxide.*

Red lead being one of the most common of pigment, is unknown, I presume, to no person. The method of manufacturing it has been described by Dr. Watson in his *Chemical Essays*, by Jars in the *Memoires* of the French Academy for 1770, and by Ferber in his Mineralogy of Derbyshire. Red lead;

1. It is a tasteless powder, very heavy, and of an intense red colour, often inclining to orange. I have never met with any specimens of it absolutely pure, but not unfrequently the foreign bodies do not exceed one or two *per cent.* They consist of seven grains of sand and oxide of antimony. Dr. Watson found traces of silver in it. It loses no sensible weight in a heat of 400°. its characters;

2. When 50 grains of red lead are digested in diluted nitric acid, they leave 12 grains of brown oxide. The solution evaporated to dryness, yields 56 grains of nitrate of lead. Now, 56 grains of nitrate contain 38.36 grains of yellow oxide. Red lead, therefore, is composed of 38.36 yellow oxide and 12 brown oxide, or, *per cent.* of contains 88 lead + 12 oxigen;

76.72 yellow oxide,
24.00 brown oxide.

100 72

The excess must be ascribed to the imperfection of our methods. I shall omit it in the calculation: Not that red lead

is a mixture of yellow and brown oxides, but that it contains all the lead and oxigen in the above proportions of these bodies. Now

76 grains yellow oxide is composed of	68.8 + 7.2
24 grains brown oxide of - - -	19.2 + 4.8
Therefore red lead is composed of -	88. + 12. = 100.

not easily decomposed by mere heat.

3. It is well known that red lead gives out oxigen gas when heated, and that it approaches to the state of yellow oxide. The loss of weight ought to give us the portion of oxigen which it contains more than is necessary to constitute it yellow oxide: But, upon trial, I could obtain no satisfactory results this way. In one experiment 100 grains of red lead lost 4½ *per cent.* in another seven *per cent.* The experiments were made in small covered earthen-ware crucibles. The oxide was melted into a dark-brown transparent glass, not unlike glass of antimony, but much harder. On breaking this glass, I found in it globules of lead reduced to the metallic state: This accounts in part for the loss of weight, and shews us also, contrary to the opinion of chemists, that the red oxide of lead is reducible, at least in part, by mere heat. In all probability nothing prevents the complete reduction but the readiness with which it unites with the vessel in which the experiment is made. I twice varied the experiment, by enclosing the small earthen crucibles containing the red lead in a crucible of platinum; but in neither case did I obtain any visible metallic globules; yet the loss of weight was the same. This renders it probable that a portion of lead had been reduced and afterwards diffused through the oxide.

Not soluble as *red lead* in acids, nor in alkalis.

4. The red oxide of lead does not seem capable of combining with acids. Many acids indeed act upon it, but they always begin by reducing it to the state of yellow oxide. The fixed alkalies do not alter its colour, but they gradually dissolve it. From this solution it is thrown down always in the state of yellow oxide: Hence it must lose oxigen during the solution.

V. *Litharge.*

Though litharge is very far from being the same with red lead, yet, as the mode of preparation is analogous, a few remarks on it may not be improper in this place.

Litharge

Litharge confifts of fcales, partly of a golden yellow colour and partly red: They poffefs a certain degree of elafticity. The method of making *litharge* has been defcribed by Dr. Watfon, by Gmelin, and by other chemical writers. Litharge;

1. When 100 grains of litharge are expofed to a red heat, they melt into a yellow glaze, and lofe, at an average, four grains of their weight. When 50 grains of pounded litharge are thrown into nitric acid, they diffolve with effervefcence, and lofe two grains of their weight. The effervefcence and lofs of weight are owing to the efcape of carbonic acid gas. From this we may conclude, that litharge contains four *per cent.* of carbonic acid. contains carbonate,

2. When 50 grains of litharge were diffolved in nitric acid, and the folution evaporated to drynefs and re-diffolved in water, $1\frac{1}{2}$ grains of a grey powder remained behind in my trials, which proved to be oxide of antimony. Therefore, litharge contains three *per cent.* of oxide of antimony. and antimony:

3. The folution evaporated to drynefs, gave 68.5 grains of nitrate of lead; but this nitrate contains 46.72 grains of yellow oxide of lead. Of courfe, we have litharge compofed of Comp. parts inveftigated;

93.44	yellow oxide of lead,
3.00	oxide of antimony,
4.00	carbonic acid.
100.44	

The fmall excefs muft be afcribed to unavoidable errors in the analyfis.

4. Fifty grains of litharge diffolved in nitric acid, deprived of its oxide of antimony, and then thrown down by carbonate of potafh, gave $52\frac{1}{2}$ grains of carbonate of lead. Hence 97 grains of litharge (fuppofing the antimony a foreign body) would have given 105 grains of carbonate. But 97 grains of litharge contain nearly four of carbonic acid. Hence we have the carbonate formed of 93 oxide and 12 acid.

In this experiment the carbonate produced was too fmall by about a grain. This was partly owing to the lofs of a fmall quantity of the powder while feparating it from the filtre. As I could not eftimate the lofs, I left it out in the calculation, and ftated the amount precifely as I found it.

From the preceding experiments it follows, that litharge is a fub-carbonate of lead, fince it confifts effentially of about 96 it is a fubcarbonate:

 yellow

96 yellow oxide,
4 carbonic acid.

——
100.

Probably it varies somewhat in the proportion of its constituents, according to circumstances: But all my trials were made on one parcel of litharge. I have observed traces of carbonic acid also occasionally in red lead, but too little to affect its weight.

VI. *Conclusions.*

Summary recapitulation.

From the preceding experiments and observations we are entitled, I think, to draw the following conclusions:

1. Three oxides of lead only are at present known. The constituents of these oxides may be seen in the following table:

Oxides.	*Colour.*	*Constituents.*	
		Lead.	Oxigen.
Protoxide.	Yellow.	90.5	9.5
Deutoxide.	Red.	88	12
Peroxide.	Brown.	80	20

Lead. Oxigen.
100 + 10.6 = 110.6 protoxide.
100 + 13.6 = 113.6 deutoxide.
100 + 25. = 125. peroxide.

2. The ashes of lead are a mixture of protoxide and lead in powder.

3. White lead and litharge are combinations of protoxide with carbonic acid: the first is a carbonate, the second a subcarbonate of lead.

* As colour is a very ambiguous criterion for distinguishing metallic oxides, I have been accustomed for some time to denote the oxide with a minimum of oxigen by prefixing the Greek ordinal number to the term oxide: Thus, *protoxide of lead* is lead united to a minimum of oxigen. The oxide with a maximum of oxigen I call *peroxide*: Thus, *brown oxide of lead* is the *peroxide* of lead. I denominate the intermediate degrees of oxidizement by prefixing the Greek ordinals 2d, 3d, 4th, &c. Thus, *deutoxide* is the second oxide of lead, *tritoxide* of cobalt, the third oxide of cobalt, and so on.

4. The

4. The yellow nitrate of Proust contains the same oxide as common nitrate. But in it there is no excess of acid; whereas common nitrate is in fact a *supernitrate* of lead. In a strong heat it is partly converted into a *subnitrate*.

5. Protoxide of lead unites with all acids, deutoxide with none, and peroxide only with hyperoxymuriatic acid.

6. The protoxide of lead may be formed by combustion; but the other two cannot, and indeed lose oxygen in a strong heat. The deutoxide is formed by keeping protoxide in contact of air at a given temperature: the peroxide by the action of nitric or oxymuriatic acid on the deutoxide.

XI.

On certain Chemical Effects of Light. In a Letter from WM. HYDE WOLLASTON, *M.D. F. R. S.*

To Mr. NICHOLSON.

SIR,

HAD I foreseen the publication of Mr. Ritter's '*Experiments on Light*' in the last number of your Journal,* I would have requested you to accompany them with a few observations of mine on the same subject; not with a view of claiming any priority in the observation of those invisible rays, that have chemical effects, which I believe occurred to Mr. Ritter and myself very nearly at the same time; but for the purpose of inserting a caution against the theory implied by the term "*disoxidating*" as applied to those rays.

Introduction on the invisible rays that have chemical effects.

In my note upon a communication to the Royal Society,† which you did me the honour to reprint in the 4th Vol. of your Journal,‡ I was careful to express the power exerted by the most refrangible rays on muriate of silver, in general terms as *chemical*, not merely from a doubt whether they would in other cases produce a corresponding effect, but because I had at that time made the following experiments, which proved that the same rays, which cause the emission of oxygen by muriate of silver, occasion its absorption by the resin usually called gum guaiacum.

The power of these rays was termed *chemical* by Dr. W. and not *disoxidating*, because they do not constantly disoxidate.

* Page 214. † Phil. Transf. 1802, p. 379. ‡ 8vo. series. p. 99.

My

My reafons for withholding thefe experiments at that time were, that they appeared fomewhat irrelevant to the primary fubject under difcuffion, and that I was alfo in hopes of increafing their value by additional trials on other fubftances.

Whether the folar rays tend to extinguifh fire; —not proved.

Upon confidering the power which thefe rays poffefs of expelling oxygen from the muriate of filver, I thought it not impoffible, that there might be more truth than I had been accuftomed to fuppofe, in the popular obfervation that the fun has a tendency to extinguifh fire, as the fame rays might retard combuftion by oppofing the abforption of oxygen. Accordingly I made various experiments on different fubftances in a ftate of flow combuftion, but without any apparent confirmation of that hypothefis.

Light or folar rays did not affect vegetable blues;—

I alfo tried the action of light on feveral vegetable blue colours, which are known to be affected by union with oxygen, and upon the fame colours previoufly reddened; but on thefe alfo I did not fucceed in producing any effect at either boundary of the prifmatic fpectrum.

Trial of guaiacum.

After failure of thefe endeavours, I had recourfe to guaiacum, which I had long known to acquire a green colour by expofure to light; but that the prefence of air is alfo requifite for this purpofe I had afcertained in the following manner.

No change by folar rays without air.

Two plates of glafs were heated with a fmall piece of guaiacum interpofed, and thereby cemented together in their centers for a circular fpace about 1½ inch diameter. In this ftate they were expofed for feveral weeks during fummer to the fun, without the fmalleft apparent alteration in the colour of the guaiacum.

—but when the air had accefs;

The plates were then forcibly feparated; and as they were both fimilarly coated with a portion of the refin adhering to their furfaces, one of them was preferved for comparifon in a dark place, where it had free accefs of air alone, while the other was again expofed uncovered to the mid-day fun.

—it was affected in five minutes by the noon-day fun.

The latter was in five minutes perceptibly rendered green, and in a few hours had acquired the full colour, which it feemed capable of receiving; but the former, in the courfe of many months that it was kept confined from the light, feemed not to have been difcernibly altered.

The prifmatic fpectrum was too weak to fhow whether all the rays were effective or the contrary.

Since by later experiments it appeared probable, that the whole of the fun's rays were not active in this procefs, with a view to determine on what part of them the effect might depend,

pend, I diffolved fome guaiacum in alcohol, and after having wafhed a card with the tincture, I expofed it for fome time to different parts of the common prifmatic fpectrum, but without producing any apparent change. It therefore became neceffary to have recourfe to other expedients for increafing the power of the fpectrum.

Over the furface of a lens 7 inches in diameter, was pafted a circular piece of paper having its radius $\frac{1}{10}$ of an inch lefs than that of the lens. I had confequently remaining uncovered a prifmatic annulus, correfponding in the length of its circumference to a prifm 22 inches long, fo arranged by its circular form that any one of the colours might at pleafure be brought to a focus, or the fpectrum might be received as a ring of any diameter required, by mere variation of the diftance of the lens. At fhort diftances the exterior margin of the fpectrum of courfe was red, and the violet within. The focus of brighteft illumination was at $24\frac{1}{4}$; at greater diftances the fpectrum again became an annulus with its colours in an oppofite order to the preceding, having the violet on the exterior margin.

A broad convex lens being covered all but a narrow exterior ring, gave, at pleafure, according to the diftance, a focal fpot of any one colour, or an annular fpectrum, &c.

With this apparatus the effect produced on muriate of filver is much accelerated. At diftances fhort of $22\frac{1}{4}$ inches a ring is produced; at $22\frac{1}{2}$ a circular dark coloured fpot; and at about 23 inches appears to be the focus of thefe rays, as the fpot is then fmalleft; at $23\frac{1}{2}$ it is larger, at $24\frac{1}{4}$ it again becomes a ring fhaded to the center; and at $24\frac{1}{2}$, (unlefs the paper has been wetted,) the center remains compleatly white though ftrongly illuminated. I have not however been able in any fituation to reftore the white colour to muriate of filver, after it has once been tinged, however flightly, by expofure to the moft refrangible rays.

Muriate of filver very fpeedily affected by thefe concentrated rays;—

but not reftored to whitenefs.

The experiments on guaiacum neverthelefs will prove diftinctly, that the powers of the two extremities of the fpectrum are not only different, but oppofite in their chemical effects.

Guaiacum was affected both ways.

A fufficient quantity of paper having been tinged with the folution of guaiacum, was cut into fmall pieces, fome of which were expofed to the fun-fhine till rendered compleatly green; the reft were kept confined from the light till taken out for each experiment.

The firft endeavour was to afcertain the focal diftance of thofe rays which gave the deepeft colour in a given time; and it was found to be about 23 inches diftant from the lens. At

At a fhort focal diftance (namely of rays moft refrangible) the deepeft green was produced;—

ſhorter diſtances the ſurface coloured was larger, but in the ſame time paler. At 22¾ a green ring was formed, having its center without colour.

At a medium focus no effect was produced; When the guaiacum was expoſed at diſtances greater than 23 or 23¼, the ſurface coloured was alſo larger, but much paler than at equal diſtances ſhort of the focus; inſomuch that at 24¾, which correſponds with the principal focus of illumination, little or no effect was produced in the ſpace of one minute, which was the time employed in other experiments. It was manifeſt therefore, that the chemical effect of the moſt refrangible rays (which were now diverging beyond their focus) was in this ſituation counteracted by an oppoſite action equally powerful, of the moſt refrangible (not yet arrived at their focus;) and as it appeared probable that the power, which could in one inſtance prevent diſcoloration, might alſo, when duly applied, remove the ſame colour after it had been produced.

At a longer focal diſtance (namely of rays leaſt refrangible) the original yellow was reſtored. I next expoſed to the condenſed ſpectrum, at various diſtances from the lens, portions of the paper that had been previouſly rendered uniformly green. A ſecond focus was now found at the diſtance nearly of 25¼, in which the green colour was compleatly removed, and the guaiacum reſtored to its pale yellow colour.

It is unneceſſary to deſcribe minutely the conſequences reſulting from variations of the diſtance, as the effects in this inſtance were neceſſarily the counterpart to the preceding; the circle of yellow was larger, when the paper was placed at a greater diſtance from the lens, and at the diſtance of 25⅞ the center remained green, ſurrounded by a yellow ring, correſponding nearly to the red and orange interior margin of the annular priſmatic ſpectrum.

In carbonic acid gas the diſoxidation, but not the oxidation was practicable. The ſame experiments being afterwards repeated in carbonic acid gas, only confirmed the opinion before entertained, of the cauſes to which the changes of colour were owing, but afforded little additional information. In this gas the guaiacum could not be rendered green at any diſtance from the lens, but was ſpeedily reſtored from green to yellow by expoſure to the focus of red rays.

The removal of colour (or diſoxidation) was alſo effected by heat from a piece of metal. Since the removal of colour was obſerved to take place in the ſituation of the principal focus of heat, it ſeemed deſirable to aſcertain whether the preſence of light, or the circumſtance of radiation had any influence in promoting this effect. A

piece of paper was therefore ſtained with guaiacum and after being rendered green by expoſure for a ſufficient length of time to light, was preſſed on its poſterior ſurface with a ſilver ſpoon previouſly heated over the flame of a candle; and the green colour was thereby as effectually removed, as in the focus of ſolar heat.

The laſt experiment may poſſibly appear to have been unneceſſary; but until it is explained why the heat, that accompanies the ſun's rays, penetrates the ſubſtance of tranſparent or ſemi-tranſparent bodies, while the radiant heat from a fire has ſcarcely power to enter even the moſt tranſparent, but principally ſcorches the ſurface, and is thence ſlowly conducted into the interior parts: no degree of caution upon a ſubject ſo imperfectly underſtood, ſhould be deemed ſuperfluous.

Radiant ſolar heat penetrates tranſparent bodies; culinary radiation ſcarcely at all.

I remain, Sir, &c.

W. WOLLASTON.

XII.

Illuſtrations of Mr. Dalton's Theory of the Conſtitution of Mixed Gaſes. In a Letter from Mr. WM. HENRY, *of Mancheſter, to Mr. Dalton. Communicated by the Writer.**

To Mr. DALTON.

DEAR SIR,

IN the firſt enunciation of a new theory, it is not unuſual that ſome links are omitted in the chain of reaſoning, which led to its formation; and thus the doctrine fails of that ready and general acceptance, which immediately follows its more diſtinct development. Such an omiſſion appears to me to have taken place in your Theory of the Conſtitution of Mixed Gaſes; for, according to your own candid confeſſion, ſeveral perſons, verſed both in chemical and mechanical ſcience, have declared their inability fully to underſtand the ſcope of the hypotheſis, and conſequently to judge of its merits or defects. In the diſcuſſions alſo, which took place in this Society, on your ſeveral papers, the doctrine was oppoſed by almoſt every member intereſted on ſuch ſubjects, and by no one more ſtre-

Cauſes why a new theory may not readily produce conviction and adoption.

* Read before the Mancheſter Society.

nuouſly

nuoufly than myfelf. Subfequent attention, however, to the evidences of the theory, and ftill more the refults of experiments which were made under impreffions very unfavourable to the hypothefis, have fatisfied me that the oppofition to it arofe chiefly from an imperfect comprehenfion of the argument; and that your theory is far better adapted than any former one, for explaining the relation of mixed gafes to each other, and efpecially the connection between gafes and water.

Diftinguifhing principle of Dalton's theory.

The diftinguifhing principle of your doctrine I apprehend to be, that *mixed gafes neither attract nor repel each other, and that every gas is as a vacuum to every other gas.* It is not my intention to recapitulate your proofs of this pofition, but merely to add to them the evidence of a few facts, which have occurred to me, and which ftrongly tend to eftablifh the fame conclufion.

Since gafes are held in water (always) mechanically in quantities as the preffure, different gafes do not prefs each other if they do not prevent efcape.

From a feries of experiments, which I communicated to the Royal Society, and which appeared in their Tranfactions for 1803, it may, I think, be fafely inferred, that the relation of gafes to water is altogether a mechanical one; for the quantity abforbed follows exactly the ratio of the preffure. If then it can be fhewn that a gas, abforbed by water, is not retained in its place by an atmofphere of any other gas, we fhall be furnifhed with a ftrong prefumption that different gafes do not gravitate on each other.

Carbonic acid quits water expofed to the atmofphere,

It is well known that water may be charged with its own bulk, or rather more, of carbonic acid gas, under a preffure of 30 inches of mercury. The gas, thus abforbed, is retained fo long as the water is preferved from contact with any other gas; but, when expofed to the atmofphere, the carbonic acid gas rapidly efcapes. Now this effect can be only afcribed to one of two caufes, 1ft, the affinity of carbonic acid for atmofpheric air may furpafs that of its affinity for water; or, 2dly, the air of the atmofphere does not prefs on the gas in the water, which is therefore placed under fimilar circumftances, as if expofed under the exhaufted receiver of an air-pump.

—becaufe attracted, or elfe becaufe not preffed by the atmofphere.

Not from attraction; for atmofpheres of different gas caufe no difference.

Were the firft explanation the true one, it might be expected that equal quantities of various gafes would detach different quantities of carbonic acid from like volumes of impregnated water; becaufe the affinities of thefe gafes, as in all other cafes of chemical affinity, differing in force, would occafion their combining with different quantities of carbonic acid, and in a

certain

certain order. But on making the experiment, with all the attention I could beftow, this did not prove to be the fact: for fimilar meafures of impregnated water gave up equal bulks of carbonic acid, to like quantities of all the different gafes.

If carbonic acid be mixed with another gas, the abforption by water is governed by the denfity of the carbonic acid and not by the nature of the gas.

The reverfe of this fact alfo occurred to me in the courfe of a feries of experiments, to which I have already referred; viz. that the admixture of common air with carbonic acid gas diminifhes confiderably the proportion of the latter gas taken up by water. Thus, when 20 meafures of pure carbonic acid gas are agitated with 10 of water, at leaft 10 meafures of gas are abforbed. But from a mixture of 20 meafures of carbonic acid with 10 of common air, 10 parts of water take only 6 of carbonic acid. That chemical affinity between the mixed gafes is not the caufe of the diminifhed amount of abforption, is perfectly clear; fince it is indifferent, as to the effect, what gas is added, and the proportion alone influences the refult. The effect is therefore to be afcribed to the diminifhed denfity of the fuperincumbent carbonic acid by mixture with another gas; and the preffure of gafes being directly as their denfity, and the quantity abforbed by water being as the preffure, the abforbed carbonic acid muft neceffarily quit the water. This efcape contin[ue]s till the carbonic acid *above* the water has a denfity equal to that *in* the water, and no longer.

Water has no preference of attraction as to the gafes.

Previoufly to my acquiefcence in your theory of mixed gafes, I undertook an extenfive feries of experiments, with a view to afcertain the order of affinities of gafes for water. But, after a great variety of trials, made with all the accuracy in my power, I could difcover nothing like a feries of elective attractions. Each gas, it was found, difplaced every other, and reciprocally was diflodged by them.

The flower efcape of a gas from water expofed to the atmofphere compared with a vacuum arifes from the lefs rapid exhauftion of the fuperincumbent gas.

It may be urged againft the doctrine of the non-gravitation of gafes on each other, that from water impregnated with carbonic acid gas, and expofed to the atmofphere, the gas ought, on this principle, to efcape as rapidly as under an exhaufted receiver. It muft be remembered, however, that the efcaping gas conftitutes, by admixture with the air of the atmofphere, a gas of diminifhed denfity, but ftill of fuch denfity as to retard the efcape of farther portions. All that the air-pump effects is to remove thefe as faft as they are liberated.

Facts explained by Dalton's theory.

There are various facts, fatisfactorily explained on this doctrine, which are irreconcilable to any former hypothefis. Of thefe

these I shall mention only a few; since the theory will receive from yourself all the elucidation that its establishment can require.

Light and heavy gases mix spontaneously.

1. If each gas be a vacuum to every other, a heavier gas should ascend into a lighter one, without the aid of agitation; and on the contrary a lighter one should descend into a heavier one. That this is actually the fact, and under circumstances very unfavourable to their mixture, your own experiments have fully proved.

Sulphuret takes oxigen from air not agitated.

2. The hypothesis explains why sulphuret of potash withdraws oxygen from the air without agitation, and whether placed at the top or at the bottom of a jar; for it acts as if the absorbed gas were the only one present in the vessel.

Absorbable gases expel the last portions of common air from water.

3. It explains why the last portions of common air are expelled from water by carbonic acid, and other absorbable gases. For these gases act as a vacuum to the air contained in the water, which must therefore necessarily quit its place. It solves also the problem how to expel completely any gas from water; for to effect this, the water must successively be agitated with portions of some other gas of the greatest attainable purity. Thus to expel atmospherical air entirely from water, it may be agitated with pure carbonic acid gas; but as the liberated common air presses on that remaining in the water, according to the proportion it bears to the superincumbent carbonic acid, the gas thus employed must be removed, and fresh and pure portions used in succession.

Best method of impregnating water with a gas.

4. By applying the same general law, we are taught how to effect the highest attainable impregnation of water with any gas. There could be no difficulty in accomplishing this object, if the gas and water were both absolutely uncontaminated by admixture with other gases; but when pure carbonic acid is agitated with water, atmospherical air is extricated, which, mingling with the carbonic acid, lessens its density. To obviate this difficulty as much as possible, a quantity of water, to be impregnated fully with carbonic acid, should be agitated with several successive portions of the purest possible gas. The unabsorbed residuum should also be very large, in order that the carbonic acid may bear a large proportion to other aeriform substances accidentally mixed with it.

These are, doubtless, only a few of the phenomena, to the explanation of which your theory may be successfully applied;

and

and I confidently expect that many facts, hitherto referred to chemical principles, will be brought, in consequence of your discoveries, within the pale of mechanical philosophy.*

I am, Dear Sir,

Your's very truly,

WILLIAM HENRY.

Manchester, June 20, 1804.

XIII.

On the Disappearance of Oxigen and Hidrogen over Water, at the Heat of the Atmosphere. By T. S. T.

To Mr. NICHOLSON.

SIR,

SOME months ago, I read in your excellent Journal an account of an experiment, which tended to shew that oxygenous and hydrogenous gases, when mixed together, and allowed to remain over the surface of water for a long time, spontaneously united and formed water. Slow absorption of oxigen and hidrogen over water.

Having long been accustomed to consider a temperature considerably higher than that our atmosphere ever attains, necessary to this union, I was naturally led to investigate this phenomenon; and for that purpose undertook the following experiments:

1. I prepared oxygen gas from black oxide of manganese, by means of concentrated sulphuric acid, aided by heat, and in order to render it more pure, I washed it well with milk of lime. I prepared likewise a quantity of hydrogen gas, by passing a few drops of water through a gun-barrel, filled with iron filings, and passed through the body of a small furnace, I introduced nearly equal quantities for both gases into a bell-glass jar, placed on the shelf of a common pneumatic trough, which stood in a room without fire, and almost without light. The mixture was suffered to remain in that situation for about five months; at the end of which time, the volume of the gases had diminished $\frac{1}{3}$.

* As the author had not seen Mr. Dalton's letter, published in our last number, at the time when these illustrations were written, he has mentioned a few circumstances contained in the letter.

2. On

Slow absorption of oxigen and hidrogen over water.

2. On reading the account above alluded to, it occurred to me that it was possible, that the diminution in bulk, might have arisen from a partial absorption of one or of both gases, by the water of the trough. In order to ascertain this, I introduced like proportions of both gases, into a jar, placed in a mercurial trough, which was in the same room with the other; and after suffering this experiment to continue as long as the other, I found that of 12 cubic inches of both gases introduced into the jar, $3\frac{1}{2}$ had disappeared; but I could scarcely perceive any moisture on the sides of the jar, owing to the small quantity of water which had been formed.

The decrease in volume, in these experiments, could not be owing to any condensation of the gases, by the coolness of the surrounding air; for I found that it took place gradually; and the mixtures were made in the beginning of January, and stood till the end of May; consequently there should have been rather an increase than a decrease in bulk, if the temperature of the air was the cause.

The residuary air contained in the jars, still consisted of oxygen and hydrogen gases; for when received into a phial, on the application of a lighted taper, a smart explosion took place, and the sides of the phial grew dim. On adding sulphuret of lime to another portion of the residue, a rapid absorption of the oxygen gas took place, and hydrogenous gas was left behind.

From these facts we may fairly conclude, that the decrease in volume was owing to the spontaneous combination of the two gases to form water.*

T. S. T.

Orkney, June 20th, 1804.

* It deserves to be considered whether the absorption of the purer gases within, and the escape at the surface of the water exposed to the atmosphere, according to the doctrine explained in Mr. Henry's paper (page 297) may not have occasioned the deficiency.

W. N.

Letter

XIV.

Letter from Dr. P. A. Nemnich, expressing Doubts with regard to the Death of the celebrated Humboldt.

To Mr. NICHOLSON.

SIR,

Hamburgh, June 29, 1804.

YOUR Journal of Natural Philosophy, &c. June 1, 1804, page 72, mentions positively the death of Mr. Humboldt, which, as we had here in Germany no other advices, I immediately communicated to my countrymen through the channel of our newspapers. There are however in Germany, as well as in France, many doubts about the validity of the said notice, and many objections made. Having quoted your Journal, as above-mentioned, I should be very much obliged to you for a more circumstantial and positive account of this report of Mr. Humboldt's death, with the day of his decease, and the way by which this notice reached England, &c. which as soon as I have received, I will instantly make public, in order to maintain that credibility your valuable Journal deserves.

Inquiry concerning the death of Mr. Humboldt.

I am, Sir,

Your's most respectfully,

P. A. NEMNICH,

Licentiate.

Extrait du Publiciste.

Paris, 20 Juin, 1804.

VOTRE feuille de ce jour contient à l'article de Hambourg la nouvelle de la mort le Mr. de Humboldt. Il m'est permis d'en révoquer en doute l'authénticité, et de rassurer les amis des sciences et de l'humanité. Je sais positivement que Mr. Guil. de Humboldt à Rome, a reçu de son frere des lettres datées de la Havane du 28 Mars, dans lesquelles il lui marquoit que sous 12 jours il seroit rendu à Charlestown d'ou il s'embarqueroit de suite pour le Havre, et qu'il comptait être à Paris avant la fin de juin.—La nouvelle est donc plus que douteuse et nous pouvons esperer, que le sort ne se feroit pas fait un jeu cruel de rendre vain le dévouement sans bornes et les nobles efforts de l' illustre voyageur. (Signé) Mendelssohn.

Extract from the Publiciste.

Extract

Extract from the Publiciste.

Paris, 20th June, 1804.

Translation. YOUR number of this day contains under the article Hamburgh, an account of the death of Mr. de Humboldt. I have reasons to doubt the authenticity of this article, and to encourage the friends of science and of humanity. I know positively that Mr. William de Humboldt received from his brother letters dated from the Havannah, of the 28th March, in which he informs him that in twelve days he should go to Charlestown and embark for Havre, with the expectation of arriving in Paris before the end of June. Your article of news is therefore more than doubtful, and we may hope that the course of events have not been so unfavourable as to render the unlimited sacrifices and efforts of this illustrious traveller of no use to society.

*** Dr. Gibbes, the author of the note in question, will, no doubt, have the goodness to mention his authority, when he sees this. W. N.

☞ *From the extraordinary Number of valuable Communications this Month, (every one of the Articles, but those of* Gay Lussac *and* Bartholdi, *being original) it has been necessary to postpone an excellent Memoir on Hauy's System, by the Abbé Briel, and a Paper by* E. O. *on the Computation of Tables of Squares and Cubes, both which, and some Abridgements and Collections of interesting Matter from the Phil. Transactions, together with the Scientific News, will appear in our next.*

Antique painting composed in Glass.

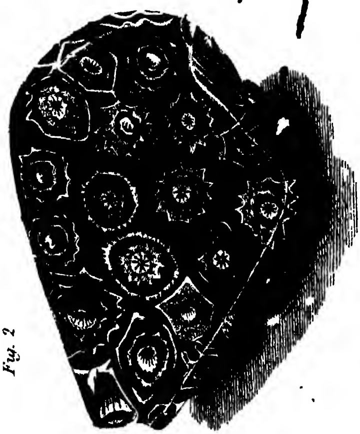


Fig. 2

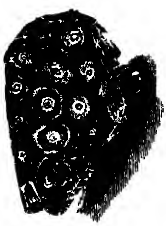


Fig. 1

Antique painting composed in glass

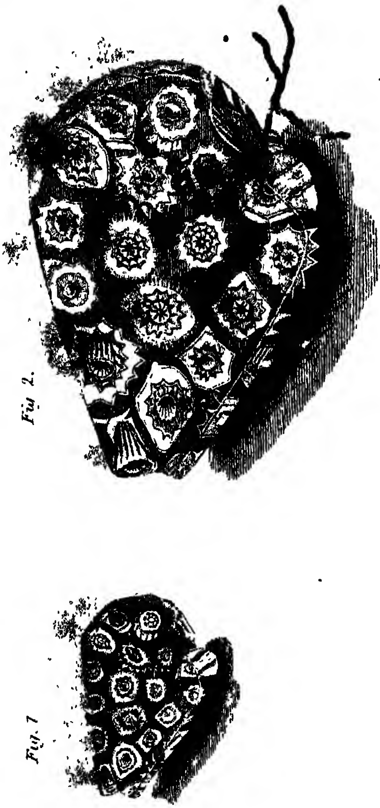
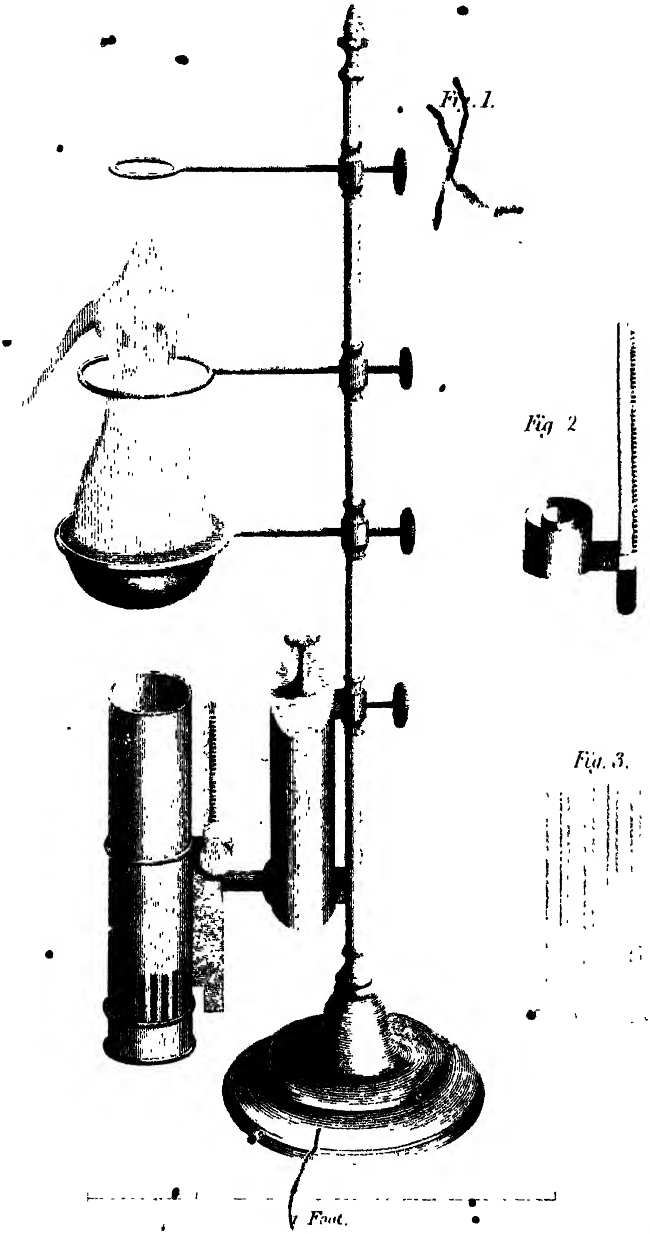


Fig. 1

Fig. 2

Mutlow Sc. Russell Ct.

Improved Chemical Lamp.



Drawn by Blunt

Engraved by Mutlow

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THE END OF THE EIGHTH VOLUME

Printed by W. Stratford, Crown Court, Temple-Bar.